The Western Journal

OF THE

MEDICAL AND PHYSICAL SCIENCES.

FOR JULY, AUGUST, AND SEPTEMBER, 1835.

ESSAYS AND CASES.

Art. I.—Tumors of the Face.

Notices of two cases of Tumors of the Face, in which operations were performed. By Horatio G. Jameson, M. D. Professor of Surgery in the Cincinnati College.

Case First.—Mr. C——, from Virginia, visited Baltimore in the month of February, 1835, with the intention of putting himself under my care, in a case of tumor of considerable magnitude. I found a tumor equal in size to a moderate sized lemon, extending from the zygomatic process of the left cheek, with its longest diameter downwards, and lying close along the ear. Neither the sight nor touch discovered any derangement of the bones internally; below the teeth of the upper jaw, there was a slight projection inward, of a small lobe of the tumor, covered by the lining of the mouth. The case had been attended from an early period with severe neuralgic pain, which might be ascribed to the pressure of the tumor on the portio dura of the seventh nerve, near its escape from the skull. Health of the patient good, except a considerable degree of debility, arising from long continued and se-

vere pain, such as to require the free use of opium. By grasping the tumor firmly with the thumb and fore-finger, there was evidently a slight degree of motion discovered, in the lower part of the tumor.

In forming an opinion, so far as it could be done by the external examination, I was led to hope, that the bones of the face were not involved in the disease; but the operation disclosed considerable absorption of the bone and muscle.

The Operation.—An incision was made commencing over the zygomatic process, and carried downwards and outwards to a point a little below the cartilagenous tube of the ear, about three inches in extent; another was now started at this point, and carried about two inches towards the corner of the mouth—the integument being dissected up over the greater part of the tumor, I now commenced at the upper portion of the tumor to dissect it out. I soon found that the tumor descended deep into the cheek, and that considerable absorption of the muscles, and a portion of the outer plate of the superior jaw bone, had taken place. Instead of finding the tumor in any part encysted, as I had hoped, it was attached to all the adjacent parts; and, when I had penetrated below the attachments to the soft parts, I found the tumor in actual contact with the bony surface. A branch of the tumor had penetrated the outer portion of the antrum highmoriamum—all of the bony surface looked healthy. The tumor readily peeled off from the bone. The maxilliary bone presented a very sharp and ragged end, where it had been resting closely against the tumor; this projection I sawed off with Hey's saw, and left a smooth, sound surface. The parotid gland, being closely attached to the tumor, was in greater part removed—several arteries were cut, and tied, or treated by tortion. Having satisfied myself, and friends present, that the diseased parts were removed, I now examined the parts with a view to dressing the wound.

When I looked at the flaps of skin, presenting an area of several inches, having nothing but the vessels of the skin, to sustain the covering for the wound, I was truly alarmed, lest the extent of detached integument was too great to allow of

a reunion of the divided parts. When the flaps on either side were turned down, and made to meet, it was seen that there would be a vast cavity between the outer covering and the bottom of the wound; these flaps passing from the upper side of the wound on either edge, and crossing over the cavity below, made by the absorption already noticed, presented a state of things requiring the most minute attention, in the coaptation of the *flaps*. Much difficulty arose from the floating or loose state of the skin, owing to its tendency to fall inward into the cavity, or descend upon the slightest pressure.

Three interrupted sutures of fine doubled thread were introduced with much care, then was commenced at the upper angle, the insertion of very fine cambric needles, at intervals of less than the fourth of an inch: and, having brought these down to the first suture of thread, the needles were now lapped in the usual manner, with fine thread, taking the utmost care to keep the edges of the cut skin precisely on a level. This, though an affair very simple to an ordinary apprehension, is really one of the most delicate operations in surgery. In the present case, the floating condition of the flaps, and extensive hollow below, rendered this unquestionably that part of the operation requiring most skill. Skill here consists in knowing how to lift or hold up the loose edges, while we at the same time pass the needle through, and so regulate it as to obtain an exact fitting between the separated edges. The introduction and lapping of the needles being completed, leaving a little space open on the lower incision, for the escape of matter, the part was dressed by applying a soft piece of patent lint, and confining it with a roller.

To my great gratification, I found on inspection on the second day, that complete adhesion had taken place throughout the whole wound, so as to require the introduction of a probe, where provision had been made for the exit of matter. A considerable discharge continued for several days, of a thin, bloody fluid—soon, however, pus began to appear, and in about ten days there was a pretty free discharge of laudable pus. The patient continued to suffer neuralgic pains of the forehead, on the affected side, but they were easily controlled

by the use of opium, and gradually his condition in all respects became more agreeable and healthful.

Two weeks after the operation, I left Baltimore, for the south, and put the patient in the care of my friend, Dr. Annan, doing well, as I believed. He left town a few days afterwards, since which time I have not heard from him.

An examination of the tumor and the improving condition of my patient, led me to hope that he would recover; but still the operation was too long delayed, and considerable deformity unavoidably followed the cure, from the deep and extensive depression left in the cheek.

The tumor was in greater part steatomatous, but traces were not wanting of the arborescent structure, so often seen in malignant tumors.

I deem this case interesting from the fact of having succeeded so well, in the reunion of so vastly great a portion of integument, without having any living structure in contact with all its inner surface.

During the swelled state of the parts the skin was tightly stretched across the cavity; but, as the healing progressed, the skin seemed to adhere to the surface below, at the edges; and gradually extended inward, so as to present a most pleasing effort of nature to effect a cure—the skin became nicely tucked in around the edge of the projecting bone, and settled down gradually to the bony surface. The approximation and union of surfaces was promoted by suitable compression fitted to the hollow.

Case Second.—I was called into Pennsylvania, by my friend, Dr. Benjamin Johnston, in the month of July, 1835, for the purpose of operating upon Mr. Strickler, residing near York. The patient is about 70 years of age, of correct habits, and firm constitution; in good health, possessed of uncommon fortitude. He has a tumor on the right side of his face, equal in size to a large orange; it occupies a large portion of the cheek, extending from the zygoma to a little under the angle of the jaw; reaching nearly from the corner of the mouth to the ear, and is closely attached to the tube of the external ear. It is obviously moveable, and the fingers can be passed

under the point of it, which creeps somewhat below the angle of the inferior jaw. There has not been much pain, and the skin over the tumor is still loose, and easily moves over the surface of the tumor, when the fingers are applied for that purpose. The thickest part of the tumor lies on the parotid gland, and we may presume, that if there be not absorption of the gland, that it is materially deranged in its function, and probably deranged in its structure. At all events, it is highly probable, that such are the attachments to the gland, and the depth of the tumor, that the latter can only be removed by the destruction of the former. In this event, the *portio dura* may be destroyed, and paralysis follow, of this side of the face.

The patient stated, with much good sense, that he had sent for me to obtain my counsel, that without regard to present suffering, he was ready to submit his case to my judgment, feeling assured that I would not operate unless it could be done with propriety.

Having carefully examined the tumor, in all its attachments, its history and character, I did not hesitate to advise its removal, because, whatever might be its course hereafter, at present it exhibited nothing strongly indicative of malignancy; and as there was every reason to hope that it could yet be entirely removed, I did not hesitate to advise an operation, seeing that ere long it would not only be beyond our reach, but death must shortly take place, since the tumor is now growing with rapidity, and principally downwards and under the angle of the lower jaw. Doctors Johnston and Jones, who were present, concurred in the opinion, that the removal was proper, as well on account of the nature of the disease, as of its practicability, though certainly a very painful, difficult, and extensive operation. Our patient had the necessary stamina, whether we allude to his corporeal or mental powers.

The patient having been made acquainted with the above view of his case, submitted manfully, and without a moment's hesitation proceeded to prepare for the operation.

The base of the tumor was irregularly circular, except a small projection downwards under the angle of the jaw.

An incision was made from the zygoma to the angle of the jaw, near the posterior margin of the tumor; another from the anterior edge of the divided skin, about two and a half inches towards the mouth. I soon discovered the tumor to be closely attached to everything with which it lay in contact, and that it required to be dissected throughout its entire surface; and, being in part steatomatous and irregular, rendered the operation tedious and painful. Being uncertain as to the condition of the parotid, and of the parotid duct, I proceeded with much caution, that I might avoid, if possible, doing injury to these structures; but, such was the depth of the tumor, so irregular its inner surface, such the confusion of parts, from pressure, induration, universal and copious hemorrhage; that I found it necessary to proceed sans ceremonie, to take out the diseased mass. The separation of the attachment of the tumor to the cartilaginous tube of the ear, and under the edge or angle of the jaw, required great care. The tumor was abundantly supplied with blood, all the smaller arteries being greatly enlarged. Some of these it was necessary to tie as we proceeded, because they were so situated that a finger could not be applied, without incommoding the operator; others were restrained by the fingers of my assistants, until the tumor was extirpated. Not less than eight or ten arteries required ligatures, and I was sensibly struck with the fact, that however well we may succeed generally, by tortion in securing arteries of the size here wounded, in this case such seemed to be the force of the circulation, that it would not restrain the hemorrhage, and it became necessary to tie every artery.

Upon examining the wound after the removal of the tumor, I discovered that there had been a very intimate union of the tumor to the parotid gland. The long continued pressure, of many years' duration, had much wasted the gland; and it is most likely that little or none of its natural function was now operative, and that the duct had been impervious. Nearly all of the gland had been removed, but since no paralysis took place, the portio dura could not have been destroyed. The carotid artery lay beating violently upon the surface of the wound, and a degree of pulsatory movement existed, at so

many points as to present rather an appalling spectacle, to see all alive with leaping arteries.

The operation, though tedious and very painful, was submitted to without a murmur, on the part of the patient; and when told it was done, he remarked that he had not expected to get so soon through the operation. Much pains were taken to satisfy ourselves by sponging and removing coagula, that all the vessels were secured. The wound was then laid neatly together, and the edges secured by a few interrupted thread sutures; then the interspace was most carefully closed by means of fine cambric needles, which were lapped in the usual manner with fine thread.

We had the gratification to see nearly all of this extensive wound healed by the first intention. There was neither palsy of the face, nor fistulous disposition; on the contrary, the parts healed kindly, with but little deformity, and in three weeks the whole wound was completely healed.

We would beseech our brethren abroad, both in this country and throughout Europe, to look at the result of this case, wherein eight or ten ligatures were applied, and union by the first intention not interrupted—and yet we are told every day, of ligatures coming away many days after application. Does it matter not whether patients be subjected to the delay, irritation, pain, inflammation, and suppuration, which always attend indissoluble ligatures? Can the profession not be brought to perceive the difference between suffering or no suffering? between safety and danger?—for most assuredly these differences exist between the animal and hard ligatures. I know of nothing in the practice of surgery so consoling, so truly gratifying, as the happy consequences attendant upon the animal ligature.

Art. II.—Unskillful Surgery.

A Brief Notice of a little bad Surgery, by a good Surgeon, wherein a Ligature was unskillfully applied. Communicated by H. G. Jameson, M. D., Professor in Cin. Med. Col.

We are told by Dupuytren, that an officer having a wound involving the temporal artery, and compression having been found ineffectual, this able surgeon cut down on the artery, and applied a ligature *on a roll of diachylon.*

Is there any difficulty in imagining that much pain, inflammation suppuration, and all these consequences having considerable duration, will follow such an operation? Is it good surgery, or is it bad surgery, to torment a patient with a remedy alike cruel and unnecessary? If this were a mere casual affair, of some ordinary practitioner, or less noted writer, we might leave it to pass for what it is worth; but, coming as it does from the highest authority, I feel an imperative call to invite attention to the case. I am the more disposed to notice this error of practice, because it seems as though the profession were benighted in regard to the choice of ligatures. Is it not enough that Physick, A. Cooper, Crampton, Travers, and many others have tested, and reported in favor of the dissoluble ligature; that the French and Germans have succeeded by the method of tortion, in saving their patients from much suffering; and especially that those who have tested the ligatures of animal skin, have found it not only safe, but more safe than the indissoluble ligatures. Is the experience of nearly twenty years, in the extensive use of the skin ligature, worth anything? If so, the present writer has, by his experiments upon living animals, and extensive application in surgical practice, shown the fitness; nay, the most decided superiority of these ligatures; and are they still unworthy of notice or imitation? Shall it be said that it is a matter of no consequence, whether we heal wounds by the first intention, over ligatures, which do not irritate and provoke unnecessary pain and suppuration; or apply cords, which, being indissolu-

ble, fret the parts as would a thorn, or splinter of wood; and after causing much suffering, must divide whatever has been surrounded before they can come away. I have seen the case of a lady, where a surgeon, who stood high in his profession, suffered a thread ligature to get under the flap of integument raised in the amputation of a scirrus mamma; and being covered in, it caused painful suppuration, which lasted with much distress for eight months, and then found its way through the sound skin by ulceration. This lady assured me, she had suffered more from this ligature, a hundred fold, than she did from the operation for removal of the breast; and much deformity followed.

In the name of Heaven, I ask the profession, wherein can lie their objections to using ligatures, which afford them facilities and advantages, that cannot be had from any others, and which at the same time, relieve patients from a large portion of suffering, to say nothing of the greater safety of the skin ligatures over all others? I have now applied many hundreds of buckskin ligatures, to arteries of all sizes, from the common iliac, down to the smallest that require ligatures; to veins in cases of wounds and operations, when it was evident that hemorrhage which existed would be attended with danger, owing to the prostration of the patient, from great losses of blood. In every instance, the ligatures answered the end expected, and without ever, to the best of my knowledge and belief, increasing the suffering, or interrupting the healing of the wound. Indeed, it is an undeniable fact, that wounds can be healed without retardation with the skin ligature; with the indissoluble, of whatever material, they cannot be thus healed.

In arteries of the first order, I have clearly demonstrated, that the sides of the vessels cannot be brought into contact, without cutting through the coats at several points, owing to the coats being so dense, that they fall into plaits by which they are subject to be cut unequally — whereas by means of a soft yielding ligature, carefully and slowly drawn, we can render the approximation more complete, and without cutting the coats at all. I truly believe, that many of the failures,

where patients died from secondary hemorrhage, where one of the larger arteries was tied, were owing to the circumstance just named; and that had an animal ligature been applied, in a skillful manner, secondary hemorrhage would not have taken place. Is twenty years' extensive experience worth anything? If it is, I repeat the solemn and important truth, that I have never had secondary hemorrhage to occur in my practice, in all that time. Surely this cannot well be ascribed to mere chance.

May I ask again, why this apathy in a matter of vital importance? Nothing is more easily procured than a piece of buckskin in this country, or chamois or other soft dressed skin in all countries, free from any impregnation with tannin. I have uniformly used soft, thin pieces of deer skin, dressed as for gloves; and cutting this into narrow strips, I have a ligature which applies with great convenience; has the advantage of never slipping, so as to loosen the first knot while a second is tying; nor will it slip off the artery, if it has the least fair hold. It becomes imbued with the lymph of the blood before we are done tying it; and is thus closely agglutinated to the vessel to which it is tied. It in a few days becomes a pulpy mass, and is carried away by absorption. So perfectly compatible is this substance with the living structures, that no irritation whatever seems to arise from its presence; such indeed is its harmlessness, that when well applied, the coats of arteries to which it is applied are never cut through, on the contrary, the vasa vasorum are suffered to continue their work of circulation. Can there be any reasonable doubt, that there is less risk of secondary hemorrhage under such circumstances, than where a harsh, indissoluble ligature is applied, which is well known to fret the surface with which it comes in contact. But apart from the reasonableness of the matter-of-fact, there has been ample opportunity for ascertaining, by observation and experience, that the animal ligature is of incalculable advantage to surgery. To be most useful, these ligatures must not be rolled or made hard by rolling them, to make them round, as was recommended by Dr. Dorsey, in his System of Surgery; nor should they be stretch-

ed except very slightly, otherwise they will cut the coats of the artery, provided they are drawn pretty tight. The most ample experience leads me to conclude with Scarpa, that so far from there being any advantage in cutting the arterial coats, as taught by Dr. Jones, it is much better to direct our measures so as to avoid cutting the coats, or strangulating the vasa vasorum of the artery. In small arteries this is not a matter of much moment, but in those of the larger classes, it is of vast importance, since it will assuredly lessen the risk of secondary hemorrhage, to apply our ligatures so that they shall not cut the coats at all.

If it be a matter of so much interest to select the best ligature, what excuse may be offered for such a surgeon as Dupuytren, who stuffed in a roll of diachylon along with his indissoluble ligature? That such was the nature of the ligature there can be no doubt, else why use the plaster. Some of Dupuytren's cotemporaries, and many German surgeons, would have treated this artery by tortion. But why not apply the dissoluble ligature, which, while it secures the vessel, is perfectly harmless? I have again and again stayed the hemorrhage from wounded branches of the temporal and occipital arteries by tortion; those of the mamma, of the fingers and toes, hands, and feet, may be thus treated, provided we can get them sufficiently within our reach, where we cannot, the skin ligature may be applied with unerring certainty. Nothing but vanity can govern men, in blindly refusing to adopt useful expedients, because they are not the inventors of them.

Art. III. — Lithotomy and Wounded Rectum.

Some Account of a case of Stone, attended with several remarkable circumstances — and in which the operation of Lithotomy was performed three times. By Horatio G. Jameson, M. D., Professor of Surgery in the Cincinnati College.

My memory has been awakened to the circumstances of this very interesting case, by reading a notice of Mr. Copland's successful treatment of rupture, between the vagina and rectum, which notice will be seen in the present number of the Western Journal of the Medical and Physical Sciences.

The subject of this truly interesting case was the late worthy Judge Sprigg, of the United States Court. This gentleman was sorely afflicted for several years with symptoms of stone in the bladder. From the commencement of severe symptoms, he endeavored to obtain the best advice in his case, he, therefore, procured the attentions of several surgeons, some of whom I do not now recollect, but Dr. Physick, and Dr. Davidge, of Baltimore, were among the number. These and others sounded the patient, so as to be satisfied, at least, that they could not find a calculus. He underwent various plans of treatment, the particulars of none of which do I recollect, till he came into my hands.

It is not my intention to go into the minutiæ of the case, I shall therefore only notice some of the more striking or peculiar circumstances and symptoms. I sounded this gentleman repeatedly in various positions of the body, aiding my efforts by occasional distention of the bladder by warm water injections. For a long time (several years) the patient was wholly unable to void his urine, and was compelled to draw it off frequently, which he did by means of a large flexible metallic catheter — there were frequent and very painful contractions of the bladder. The patient had not, during the use of the catheter, been sensible at any time of touching a stone. During my soundings, I told the Judge I had perception of

something like a stone twice, but so faint as by no means to enable me to say there was a stone; indeed, this inquiry was rendered very difficult, on account of a spasmodic working of the neck, or some other part of the bladder, by rubbing so as to produce a ringing scraping kind of sound, against the sound or catheter. There did not appear to be any symptoms indicating serious disease of the prostate, although both this gland, and the entire neck of the bladder, were considerably enlarged. Upon the whole the patient was suffering greatly, from all the usual symptoms of stone, and yet no stone could be detected.

I recommended the frequent injection of warm water into the bladder, on the view that if there might be stone, it must either be partially encysted, or pocketed in some part of the bladder; or in the absence of stone, the bladder might have acquired, from long irritation, a contracted and fixed form of the muscles, by which contraction and dilatation of that viscus is effected. In the first case, the gradual distention of the coats might tend to loosen a calculus so situated; and, in the second, afford relief by distention of the coats, and thus remove the morbid sensibility which caused the contraction and pain of the bladder, as we sometimes see in painful contractions of the sphincter ani, in which, from abrasion, or great irritability of the termination of the rectum, the muscle contracts, so as to cause the most exquisite pain, and in which cases separation or dilatation is the most certain and immediate means of obtaining relief.

After pursuing this course a few weeks, it fell in the Judge's way to call on the late Dr. Gill, of Hagerstown, who had acquired the reputation of a dexterous and successful operator for stone. It so happened that this gentleman discovered the stone plainly, so easily as to excite surprise, that he should be so much more successful in finding what could not be found by so many well practised hands; but to my mind there is not a shadow of doubt, that this greater good fortune was entirely owing to the altered condition of the bladder, from the employment of the warm water injections, by which the blad-

der had no doubt been gradually distended. Drs. Davidge and Wright could readily distinguish the stone after Judge S. returned to Baltimore to undergo the first operation.

This happy discovery induced Judge S. to employ Dr. G. to perform the operation for the stone, which none but himself could, prior to this period, discover. The patient, however, preferred coming to Baltimore, where he might have the advice of Dr. Davidge. The operation was performed in the presence of Drs. Davidge and Wright. Some calculi were extracted, number not recollected, but the operator had the misfortune to cut the rectum low down; another proof, we may suppose, that "the race is not always to the swift, nor the battle to the strong," since Dr. G., at that time, had acquired a high reputation as an operator.

The patient finding himself in a most deplorable condition, went on, as soon as circumstances would admit of it, to Dr. Physick, in a few weeks, I believe, after the operation.

After some considerable time, (some weeks,) this gentleman returned to Baltimore, and put himself under my care, now much inclined to, if not decided in the opinion, that among his misfortunes was that of having been, by a strange course of events, thrown out of my hands. Of what took place in Philadelphia I recollect but little, having made no notes of it, although the Judge made me fully acquainted, at the time, with all the circumstances. It may suffice to say, that I was informed that Dr. Physick repeated the operation, and took away a wineglass full of small calculi. The relief from the operation lasted but a few days, when he was shocked to find that he had gained nothing by the second operation. It had been his misfortune, to use his own language, "while he was intent upon obtaining the best skill of the country, to find that he had been, by some strange fatality, kept from obtaining the advantages expected from the most common operating surgeons."

Candor knows no distinction, and I shall not therefore abstain from expressing without enmity, or affection, as my opinion, that, if a proper operation had been performed, the

patient would have been cured by such operation, followed with skillful attentions. The operation was performed in the usual way, excepting that the fistula in perineo, which existed after the first operation, was made a part of the lateral incision. Now I am perfectly persuaded, that if the incision had been begun at the opening in the perineum; and carried down the sphincter ani, so as to throw the whole of the fistulous opening into one wound, and thus relieve that wound from the evils arising from the contractions of the sphincter muscle, it would have healed up kindly, and thus removed one of the most deplorable cases within the pale of human suffering. That this is not an opinion formed at this late period, will be proved by the method of operation adopted by me afterwards in this case, several years ago. But in addition to this, it is clear to my mind, that the calculi were not all removed in either the second or first operation, and this was the confirmed opinion of my talented and observing patient.

Judge Sprigg determined on putting himself under my care before he left Philadelphia, but dispirited as he was in mind, and broken down in body, he could not consent to a third operation, which it was evident would alone afford a ray of hope of final recovery. To this his mind was made up, but he wished to regain a portion of strength, which would enable him to undergo another severe operation. Under this state of mind he resolved upon visiting the Berkley Springs, with a hope of improving his general health; but, as I had often predicted, the traveling, together with the deplorable condition to which he was now reduced, continued more and more to prostrate him. Seeing that death was advancing daily more near, he, with great difficulty returned to Hagerstown, after being some weeks at the Springs. Arrangements were made for me to meet him, for the purpose of giving him the chances of another operation at that place.

Previously to stating the Judge's condition at this time, it seems proper that I state what was his condition when he returned from Philadelphia. He could now discover evidence of stone at almost every introduction of the catheter, by

which alone could he be relieved of the urine. At very short intervals he suffered from the most agonizing spasms of the bladder. His bowels were torpid, and required frequent aperients; whenever he had a passage the more liquid fæces would pass, with spasmodic violence, into the urethra, through the opening made into the rectum in the first operation; and, such was the arrangement of the parts that, notwithstanding a free opening in the perineum, the irritation given to the ejaculator muscles, by the stimulous of the fæces, was such that they were driven from the meatus urinarius with great violence — what with the extreme pain and disgust arising from this deplorable state of things, this gentleman's situation was truly wretched, for, in addition to what we have just detailed, the fæces would at the same time pass through the natural outlet, and through the opening in the perineum and end of the penis.

The patient having made his arrangements for my meeting him at Hagerstown, he with great difficulty reached this point, after enduring much suffering by the way. I now found him greatly emaciated, and suffering every half hour with agonizing spasms of the bladder, attended by incessant hiccup, also peculiarly spasmodic, and so loud as to be heard out of doors, as the patient lay up stairs; there was now extreme debility, and inability to take almost any food. Such, indeed, was the state of the case, that I unhesitatingly told him that I could not allow myself to believe that the operation could now afford a prospect of relief, it being even doubtful whether his strength would enable him to undergo the operation, without risk of dying in our hands.

After careful reflection I was called to the bed side, when my patient expressed himself to this effect: I am a most afflicted sufferer; I strongly desire that an operation may be performed, under the hope of obtaining even temporary relief of short duration: you, in whom I have much confidence, and after you have encountered the fatigue of a long journey, desire me not to undergo the operation, to insist upon it, under such circumstances, would be little better than suicide, what then shall I do? ever so short a respite would be accept-

able, nor would the operation be attended with more severe pain than I am almost constantly suffering.

Placed in these trying circumstances, and aware that Dr. Dorsey and Dr. Reynolds, who were present, preferred leaving the decision entirely to myself, I requested further advice, provided there was any within reach which was thought worth obtaining. Dr. Harrison, of Martinsburg, Va., had spent some time at the Springs, with Judge Sprigg, and had paid some attention to his case; had obtained the Judge's confidence, no doubt deservedly. Dr. H. was immediately sent for.

The consultation having been assembled, Dr. Harrison at once gave as his opinion, that we ought to comply with the Judge's wishes, inasmuch as he could not possibly loose by the operation, and from it there was a possibility of relief; without its speedy performance a lingering, painful death must terminate the case. Sustained then by the opinion of Dr. H., and supported by Dr. Reynolds, and Dr. Dorsey, I at once proceeded to the operation.

I had a capacious urethra, a fistula in perineo, and a fistulous opening between the urethra and rectum, just behind the sphincter ani, to deal with. However little chance of recovery, this operation should be so directed as to obtain, if possible, a cure of these fistulæ — if this can be effected without prolonging or increasing the sufferings of my patient, duty requires that my measures be so directed.

It fortunately occurred to my mind, that by proceeding so as to include these fistulæ in my incisions, I should effect all that was requisite to cure these annoying sores. A straight director was introduced into the anus, through the recto-urethral fistula; and its end brought out through the opening in the perineum — a bistouri was made at one push to divide all the parts external to the director. This afforded ample space to admit of the forceps, and extraction of any calculus of moderate size, and it was already known, by the soundings, that there was none large. A director was now passed from the perineum into the bladder, and a scalpel passed in along

the groove of the director, until I was satisfied I had made a division of sufficient extent. The forceps being introduced, a few calculi were removed; not finding any more, the fore-finger was introduced, and I soon discovered a very singular state of things. The prostate, or rather its middle lobe, together with the neck of the bladder, on its inside, were greatly thickened or enlarged, and projected a knob-like tumor into the bladder. The bas fond of the bladder had by some cause been pushed down between this projection and the rectum, so as to form a depression, or pocket, into which had fallen a great number of small calculi, and where, no doubt, these bodies had long been lodged, and where they were no doubt concealed during former soundings, and former operations, not those now present, but a succession of them, and I suppose it was only those that rose up out of the pocket, so as to come under the touch of the sound or the forceps that were felt or extracted. I proceeded to turn the point of my finger round the projection, so as to carry the point to the bottom, then straightening the finger, while I kept the upper side of the end of my finger, or nail, against each calculus, I could thus raise them out of the pocket, and pass them up into the upper part of the bladder, and there reach them with the forceps. I went on to raise a number at each introduction of the finger, and then removed them by the forceps. I proceeded until I was fully satisfied that I brought every stone out of the pocket. The forceps of Dr. Barton were used, and with them several of the calculi could be taken out at once. The operation finished, the patient was put to bed, much less exhausted than could have been expected. Indeed, he was greatly relieved by the operation, and until the termination of the third day, there were strong hopes of recovery. He was in a great degree freed from suffering, but the constitutional powers were worn down, and at the end of the fourth day Judge Sprigg's case terminated with his life.

Dr. Reynolds counted the calculi, and they were found to amount to forty-nine, besides a good many pieces—they might be resembled to a collection of Lima beans, supposing them to

be thrown together from the smallest to the largest sizes of the ripe beans, and were equally flat.

The fact of there being now found such a number, so soon after two operations, goes to prove that the pocket above noticed had never been emptied before; besides the peculiar derangement of the bladder, renders it highly probable, that these calculi had, in greater part, been left undiscovered until the last operation.

It may confidently be asserted, that so far as the incisions were concerned, this was the least painful operation that was ever performed of its kind. One incision external, and one push of the scalpel into the bladder, constituted the whole of the cutting; and it is not a little curious that by one incision, the necessary and proper operations for fistula in ano, fistula in perineo, and the perinæum laid open as the first step of the operation of lithotomy, were all accomplished by one incision.

I would especially recommend, should it ever be the misfortune of a surgeon to cut the rectum any where near the sphincter ani, to divide that muscle as recommended by Mr. Copeland and Mr. Brodie, in cases of recto-vaginal fistulæ. And we feel assured that if the patient whose case has just been detailed, had had a little more strength to enable him to struggle against his disease, this simple method of treating his case would have cured the fistulæ; nor do I imagine that his life was shortened by the operation; on the contrary, there is much reason for believing, that he would have died two or three days sooner, had it not been performed, and would have suffered much more.

Art. IV.—Lithotomy.

Desultory Observations on Lithotomy, with Remarks on some Defects in the Common Gorget. By Joseph N. McDowell, M. D., Professor of Special and Surgical Anatomy in Cincinnati College.

The operation for stone in the bladder has from the infancy of the profession of surgery, been viewed as one of the most formidable to which the human frame can be subjected, and therefore, as one of the highest achievements of the practitioner in the art.

The reason the operation has been thus considered, and approached with so much terror, by both patient and surgeon, must be obvious:—the parts through which the instruments employed are to pass, being both delicate and complicated, and the bill of mortality probably greater in this than in any other operation.

But the history of the operation of lithotomy will most conclusively teach us, that the want of success has been justly attributable to the want of proper instruments, or the malconstruction of those employed, rather than to a deficient knowledge of the parts concerned in the operation. Although these parts were described with much accuracy by Celsus, his history of the operation and the result, conveys the idea that it was rude and unsuccessful, because of the surgeon's being unable to use with any certainty his instruments. The plan first devised, and termed the Celsean operation, (though originating in the school of Alexandria several hundred years before,) was the simple incision on the stone, when pressed against the perinæum, by the introduction of the two first fingers of the left hand into the rectum, and grasping the stone, and forcing it down. The want of success was, doubtless, owing to the great uncertainty as to what parts the knife would pass through, for much must have depended on the form and size of the stone, as well as the direction towards the perinæum, given it by the fingers. Thus parts were often

injured, the restoration of which could not readily be effected, and cutting for the stone was almost as fatal as the malady itself.

The introduction of this operation by the most learned and classic of Roman authorities, tended much to its perpetuation in Europe, and, although rude, it was practiced for more than thirteen hundred years.

But the writings of Hippocrates, which were brought into Europe at a much later period, and the implicit confidence extended to his aphorisms, gave rise to another mode of extracting the stone. The proposition was, to remove the calculus by dilating the urethra, inasmuch as the father of medicine had declared, that wounds in the membranous parts of the body were mortal, and for this purpose a variety of instruments were constructed, the number and peculiarities of which have not been surpassed by those employed in any operation either before or since. The remarkable difference in the number of instruments used in this mode of operating, and that advised by Celsus has given the name of the apparatus major to the one, and that of apparatus minor to the other. In the latter, only the knife and hook are used, while in the former a small volume would scarcely suffice for their description.

The absurdity of this operation, soon, however, brought it measurably into disuse; for while its advocates contended that the urethra should not be wounded with the knife, but that it was susceptible of a dilatation sufficient for the passage of any stone, they not only cut into the membranous portion of the urethra, but by the rudeness of their dilators, lacerated the prostate gland and neck of the bladder; so that the mortality was far greater than was attendant on the operation of Celsus, or cutting on the gripe, as it has since been termed.

In many instances the neck of the bladder has been torn from the prostate gland, or the urethra severed from both, and so great the consequent irritation and inflammation, that death has speedily ensued. And we are informed by S.

Cooper, that "notwithstanding these serious results, this mode was practiced by Pare, Le Dran, Le Cat, Mery, Morand, Marichal, Rau, and many of the best surgeons of Europe, for several hundred years," down to the introduction, into Paris in 1697, by Frere Jaques, of the original model of the operation of lithotomy, and the invention of the lateral operation by Franco, which has undergone so many modifications and changes, both in Europe and America; and which at last seems to be the only mode by which a patient with stone can with safety and certainty be relieved.

Fabricius Hildanus, it appears, first used the round staff in lithotomy, and it was by this means he attempted to obviate the difficulties which presented themselves in the operation of Celsus. By making the staff prominent in the perineum, he cut down upon it, and with much more certainty than in the original mode; and many surgeons, even at the present day, think this a safe operation, especially in children.

Frere Jaques, adopting the staff of F. Hildanus, performed his operation by first making the convexity of the staff prominent in the perineum and plunging in the direction of the bladder, an instrument, long and dagger-like, making it enter near the tuber ischii. This singular individual appeared in Paris in 1687, and his comparatively wonderful success, at least the extravagant number of his patients, for a time attracted the attention of all Europe, and many of the most distinguished surgeons adopted his mode of operating. His instrument may be justly considered the first great step in the construction of our present gorget. The improvements of this mode of operating, by grooving the staff and putting a beak on his instrument, by the Parisian surgeons, brought this plan almost to equal the present mode of operating with the gorget. But for a long period there was no cutting edge to the poignard-like instrument, and all the openings in the bladder were made either in the form of a lacerated or punctured wound; and so great was the terror excited by the flow of blood in the minds of the older surgeons, that it was not until the days of Sir Cæsar Hawkins, that the gorget was im-

proved by giving it a lateral cutting edge, which suggestion has proved to be far the most important that has been made in the operation, and the success attending the use of the instrument thus constructed much greater than that of any previously invented.

Since this period, many attempts at the improvement of the gorget have been made, both by the continental surgeons and those in England; but their suggestions have not added much to the facility or safety of the operation. Cheselden performed this operation with various modifications in the form of the instruments; and the present English mode of extracting the stone, both by the gorget and the knife differs but little from that so repeatedly performed, and with such unusual success, by that accomplished anatomist and surgeon.

The instrument preferred in the United States is an improvement on Cheselden's and Hawkins' gorget, by Dr. Physick. This gentleman, with his usual acuteness, discovered that much of the difficulty in the introduction of the instrument was occasioned by the want of a well-sharpened edge at the point of attachment between the beak and the cutting gorget, and his improvement consists in making the instrument so that the blade can be detached from the beak, that it may be better sharpened, and made to cut with greater ease and certainty when applied.

Such have been the various steps in the improvement of the plan of an instrument, which we think deserves the preference by the profession, over any other yet invented. And although the advancement has been but slowly made, yet it has always been in the most certain, and in by far the most successful track. Even the rude operation of Celsus, and that of Fabricius Hildanus, and Frere Jaques, are not now surpassed, as to safety and success, by many of the modern modes in the distinguished European schools.

In France, as theory after theory in pathology has been propagated, and each for a time has had its reign, so in surgery, instrument after instrument has been invented, for the operation for stone, and the bladder has been entered at every

accessible point, but never with a success that has equaled the old lateral operation with the gorget.

The *lithotome cache* was for a time applauded. Frere Come has extolled his high and low operation. Dupuytren has performed various operations on the bladder, and among others the extraction of the stone through the rectum. Lisfranc, and a host of others, have proposed modes and instruments of their own, but not to the advancement of the art. Civiale has caught the stone in the bladder and bored and ground it to powder; while Heurteloup has by force broken it to pieces. But the reports and confessions of the latter gentleman rivet conviction on the mind of every intelligent surgeon, that instead of being improvements in the profession, they are but calculated to torture the patient, and greatly add to the bills of mortality.

"The beautiful apparatus of Civiale (says an English writer of high distinction) was hailed as a means of doing away entirely with any other proceeding. Some ingenious alterations were made upon it by Mr. Heurteloup and others, and at one time it was confidently asserted, that almost every patient suffering from stone could obtain a perfect and a permanent cure. Some new apparatus, (I shall not pretend to say who has the merit of the invention, for it would not be safe to interfere with the contending parties,) certainly of a more efficient kind, was introduced, and forthwith the other was, by those who had previously given an opinion, denounced as totally worthless and inefficient.

It is too true that such is the case, that you might bore holes in almost any stone without in any way advancing the patient's recovery; on the contrary, with the effect of superadding to his other maladies a thoroughly diseased bladder. This stone (he continues) was removed from the bladder, by lithotomy, in one-tenth part of the time that any of the sittings had occupied, and with certainly a fourth of the pain, and with much less danger. A very small and soft stone may be managed by this drilling apparatus, but such stones bear but a small proportion to those that are totally impracticable.

It is a matter of much astonishment that some of the ingenious gentlemen, who have busied themselves about this matter, should not have proposed introducing a charge of gunpowder into one of the perforations, and thus shattering the concretion, as rocks are blasted in the bottom of the ocean. Many people are still racking their brains, to my knowledge, to invent some apparatus superior to any used.

This is all praiseworthy, but expectations have been raised too high on this subject, by far, by unwarrantable assertions. I have practiced all the operations in a wide and extended field, and have seen others perform them, and if I might be permitted to offer an opinion on the subject, I shall say that unless the laws of the human economy are subverted, a permanent cure can not be expected to follow lithotrity unless in very favorable cases, and among patients who present themselves for relief from the pains of stone, certainly not more than one-sixth ought to be submitted to that proceeding, *and would not be* by a conscientious surgeon, who could equally as well cut out the stone as to powder it down."

An allowance should, however, be made for the hostility which exists between the French and the English surgeons. But, when all that justice would give the former is awarded, we shall find ample ground for believing, that cutting for the stone is far preferable to any other mode of treatment.

Independently of the difficulties which are so apt to arise from the growth of other stones from the fragments of stone left in the bladder, forming nuclei for after depositions, the degree of irritation that attends the introduction and long retention of the instruments for the destruction of the stone, is so great, that a large number of the patients of those who practice this mode, perish from this cause alone. They either sink during the series of operations — the nervous system being unable to sustain itself against the unceasing attacks of an adversary so formidable — or inflammation supervenes, which soon hurries the tortured victim to the grave. No discussion, however, need be entered into as to the propriety of cutting for the stone, compared with Civiale's or Heurteloup's mode

of operating, since the great mass of the enlightened surgeons both of Europe and America, have facts and reasons amply sufficient to cast the balance in favor of either the gorget or the knife. This last mentioned mode of operating with the knife, (which among some of the most eminent modern surgeons has had very able advocates,) deserves much consideration, since it disputes the ground so long and justly claimed by the gorget, with more reasonable pretensions than any other method.

In the hands of a good anatomist and a judicious and careful surgeon, the knife is certainly safe; but in the hands of the inexperienced, it assuredly is a dangerous instrument.

There are, however, many objections which might be urged against the use of the knife, and we conceive that most of those which are, with great propriety, made to the gorget, as it is commonly employed, do in fact lie against the knife.

The hazard which the patient runs in the use of both, is the wounding of the rectum, or fundus of the bladder; and assuredly the one will as readily effect this injury as the other, when improperly directed. Besides, the knife in fat subjects, because of the great depth of the incision, cannot be directed with the same certainty as the gorget. It cannot be otherwise than extremely difficult for the surgeon, in *such* cases, holding the extremity of the handle of his knife, to direct it as he would desire. Thus it often happens, that the edge of the instrument is turned downwards and the vesiculæ seminales are wounded, or the neck of the bladder is mangled by the frequent attempts to enlarge the opening; and it likewise frequently occurs, that at the moment the knife has cut through the ligaments, the thickened prostate and neck of the bladder, like the gorget it will make a sudden plunge, and if not properly directed, may readily wound the bladder and rectum.

The object of this paper, however, is not to discuss at large the advantages and disadvantages of these instruments, but to offer a point for the improvement of the gorget operation.

In the study of the parts concerned in lithotomy, we were deeply impressed with the great danger to which a patient is

subjected, in the performance of this operation, with the ordinary instruments. But the practice of the operation, and a knowledge of the sad results which so often follow it, directed still more our attention to the subject, and induced us to make the attempt to obviate the difficulties, and remove the obstacles in the performance of the operation with the gorget in the present mode, and render it safe and successful, even in the hands of the inexperienced surgeon. How far we have effected our design will be seen by a reference to the subjoined plate, which explains the plan of the operation, and the alledged improvements in the structure of the instrument.

EXPLANATIONS OF THE PLATE.

A.—The grooved staff—which should be made of silver, because of its being less liable to be broken in the operation than the steel

B.—The entrance into the groove a short distance above the convexity of the staff, that the knife can with greater certainty cut down upon it.

C.—The extremity of the groove in the staff.

D.—The ball on the beak of the gorget—which should be so constructed as to screw on the beak: That when we would operate on a child, a smaller staff and blade could be employed.

E.—The central part of the gorget to which the blades are applied, is made of spring steel, that the beak, which is necessarily small, may not be broken by the force employed in operating

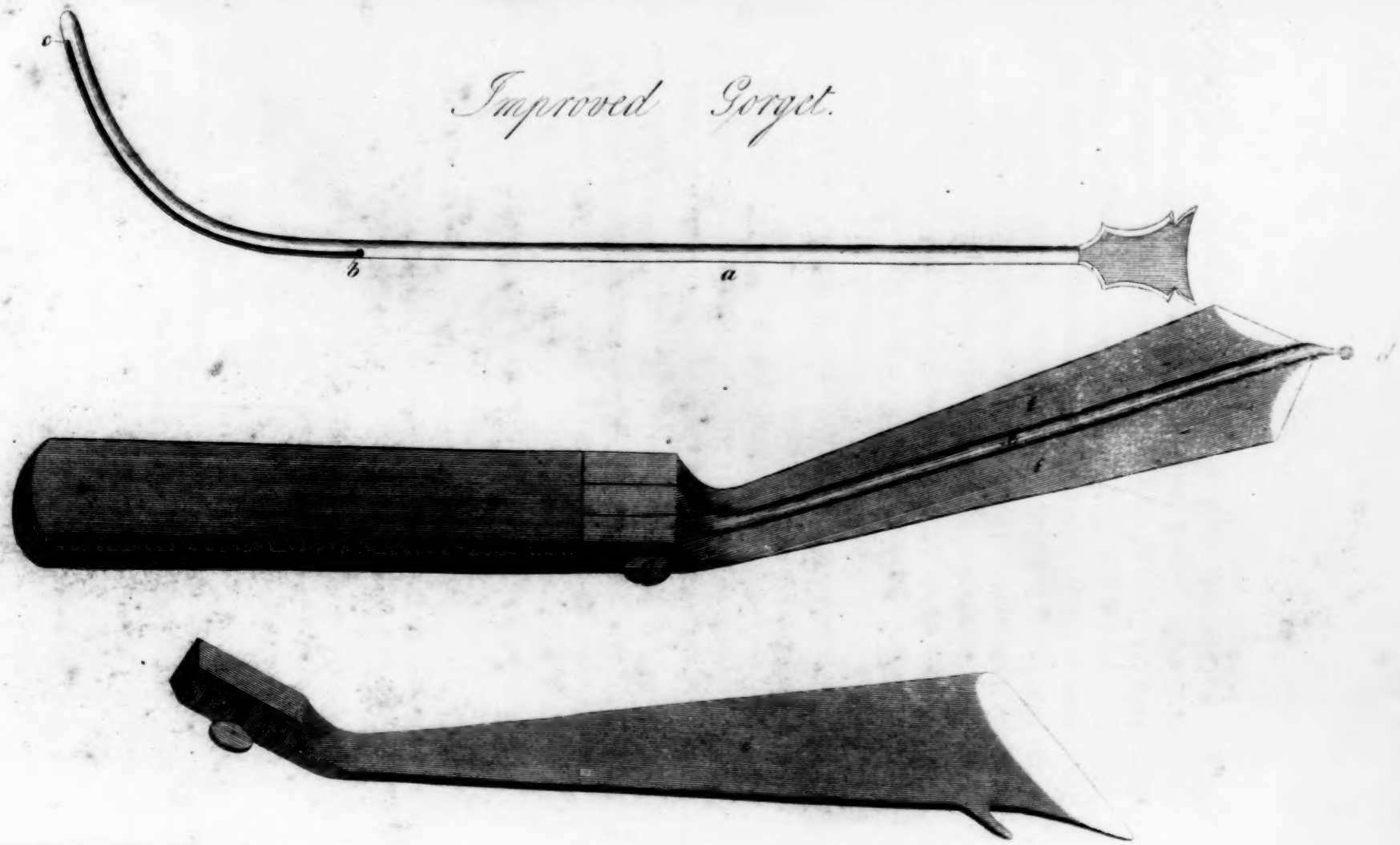
F.—The blades employed in the bilateral operation, and which can be removed for others, or for the purpose of operating with the single blade.

G.—The screw which passes through both blades.

H.—The handle.

I.—The blade detached, and made according to Physick's improvement.

Improved Gorget.



To those who are at all acquainted with the dangers of this operation, we need not offer an argument or a reason for this instrument, for its use and necessity are, at the first glance, obvious; and every operator knows how great are the difficulties attending the gorget in common use, and how much they need to be overcome. There are but few operators with this instrument, (although the best,) who, if they have used it extensively, do not have reason to complain of some accident, and would ascribe their failure to an imperfection in its formation; and who, if they spoke in candor, would not allow that some had fallen victims to its unfortunate application.

In the use of the gorget the great danger is, its slipping from the grooved staff, and injuring the surrounding parts. Thus, when the beak of the instrument is applied to the staff at an acute angle with the handle, mounting up towards the staff, if the instrument is pushed forward into the bladder in the direction originally assumed, often passing the greatest convexity of the grooved staff, it will be found to leave the staff entirely, and pass unguided into the bladder, and often into the rectum, or between the bladder and the bowel; and when the gorget is applied at an obtuse angle, it is then not so easily passed along the groove, and when too much force is applied, it has sometimes slipped upwards and forced itself between the pubis and bladder, doing great damage in that direction.

The following statements, as made by two of the most distinguished English surgeons, will suffice as proof of the dangers attending the most judicious use of this instrument.

"If I were asked," says Sir Astley Cooper, (a man whose works are characterized by honesty and candor,) "how many times I have known the gorget slip and pass between the rectum and bladder, I should say at least a dozen times, and in each case the most lamentable and fatal consequences ensued, for the operator now lays hold of the stone and the bladder together, the forceps slip, the stone enclosed in the bladder is again laid hold of, and thus he continues to pull, bruise, and injure the bladder, until the patient is taken back to his bed

with the stone unextracted. Violent inflammation supervenes from the injury done to the bladder, and in a few days the patient is no more." Sir James Earle remarks, "I have more than once known a gorget, though passed in a right direction, pushed on so far and with such violence as to go through the opposite side of the bladder." And Mr. S. Cooper asserts, he has known of at least three cases in which the gorget slipped from the staff, and the urethra was completely severed from the bladder, and in all the patients died; and it is even recorded, that when the blunt gorget has been used, that such force has been employed, that the opposite wall of the bladder has been so much torn as to allow the bowels to pass down into the internal incision.

To avoid these accidents, is the design of the instrument here proposed. The staff is so constructed as to confine the beak of the gorget, from the point at which it is applied to the extremity in the bladder. If it is necessary to the safety of the patient that the beak of the gorget should traverse the grooved staff, there can be no good reason why it should not be confined in the groove.

If this be properly done, the veriest bungler may perform the operation without the slightest danger to the patient. But an objection has been urged to the gorget, that an opening sufficiently large can not be made for the passage of many stones in the bladder; for this purpose our instrument is so constructed that two blades may be applied, and the bilateral operation performed, which is far preferable, in our opinion, to cutting entirely on one side of the neck of the bladder, for the purpose of enlarging the opening. When the two blades are applied, an opening sufficiently large may be made for the extraction of a stone as large as could possibly be taken through the perinæum. But in most instances by the use of the forceps, invented by Professor Jameson, for breaking up calculii, very large openings need never be made, at all events not larger than are required for this instrument's introduction.

In the performance of lithotomy with this instrument, some have thought it would be difficult to make the incision cor-

respond with the cut in the perinæum, but nothing can be more easily effected. Should we cut upon the staff below the opening for the reception of the ball on the beak, by which it is confined to the staff, all that is necessary is to pass the staff a little farther into the bladder, and the object is accomplished; or should we cut above, which is hardly probable, a slight withdrawel of the staff will immediately expose the place of entrance; nor are we deceived as to the certainty of its being in, as in the case of the present gorget, for so soon as we have passed it the distance of half an inch, it is confined, and cannot make its escape but by being drawn back by the operator, to the point at which it entered.

Sir Charles Blake and Le Cat are said to have devised some such instrument; but not having seen either their drawings or descriptions, I cannot tell on what plan theirs were constructed. That of Sir Charles Blake was, however, according to Mr. S. Cooper, not intended to confine or arrest the gorget at the extremity of the staff, as in the one now proposed. Thus, the dangers which have been so much dreaded could not be fully prevented, for in the plunge of the gorget into the bladder, after having passed through the thickened and often indurated prostate gland and the neck of the bladder, the rectum was as liable to injury as when the common old gorget is used, and nothing was gained by its confinement, but in its passage through the prostate, which does not constitute the danger most to be guarded against in this operation.

Mr. B. Bell, in his System of Surgery, also speaks of some invention of this kind, but objections were made to the proposed mode of operating, because of the difficulty in passing the gorget into the bladder. The instrument referred to, could not have been constructed as ours is, for not the least difficulty is found in its introduction, and the beak of the gorget is so much embraced by the staff as to bring the cutting edge so close to it as entirely to obviate the objection so much urged against the gorget—that of tearing the urethra in its introduction.

In performing lithotomy with the beak of the gorget confined, we have, also, another great advantage in avoiding the

pudic artery. We are directed to incline the staff to one side when there is danger of cutting this artery, and by being too fearful of this result, we are apt to press with so much force as to dislodge it from the groove, and it is then thrust into the bladder without any guide. But when the beak is confined, we can press with as much force as is required to pass by the pudic entirely; and, beside, the staff need not be pressed with such violence against the pubis, or so high up in the arch of the bones as to bring the blade of the gorget in contact with the artery; for should we even thrust the gorget in the bladder, with the staff pressed low down in the perinæum, we cannot wound the rectum or the fundus of the bladder. The cutting gorget is prevented from going so near the end of the staff, as by any possibility to cut the bladder, for the end pushes the parieties of the organ before and from the edge of the instrument, and, notwithstanding there may be great force employed, and after the gorget has cut through the prostate and neck of the bladder, a plunge may follow, even should it be towards the fundus of the bladder, or the rectum, the staff may be carried with the cutting gorget in such way as will cause both instruments to slide on the surface of the bladder without wounding it. A trial of this instrument on the dead subject will convince any one of the facts as stated.

In suggesting these improvements to the profession, I am not at liberty to *claim* the award of originality; for although the instruments which I have had constructed, were, as I supposed, entirely of my own invention, I since find that others had entertained similar views before me. Neither their plan nor instrument, as described, could, however, accomplish what we propose and what we candidly believe, will be experienced by all who will venture the trial.

Finally, we would not say of the invention, that it renders a knowledge of the anatomy of the parts concerned unnecessary; but much less would be sufficient.

Cincinnati, September, 1835.

Art. V.—Injury of the Brain.

Case of Fracture of the Cranium, with depressed bone, in which there was considerable loss of Brain, successfully treated. By Dr. Philip J. Buckner.

On the 15th of January, 1833, I was called on to visit the son of Capt. Daniel Holden, of this county, a lad of about fifteen years of age. Eighteen hours previous he was thrown from a sleigh, the horse in full speed, and was dashed with his head against a tree. I found him in a perfectly comatose state—stertorous breathing—paralysis of the left side—cold extremities, with slow and feeble pulse—and dilated pupils. Had vomited several times during the night. I shaved the head, and found a large portion of depressed bone, covered by an extensive ecchymosis. I sent to Augusta, Kentucky, the nearest village, for Dr. Mackey, an aged and respectable physician, to assist me in the operation of trephining. Owing to unavoidable engagements he could not attend. I then, with the assistance of two of my pupils, Messrs. Rees and Woods, commenced an examination of the wound,—divided the integuments, and dissected up, so much as was necessary to ascertain the extent of the injury, and afford room for the application of the trephine, on firm bone.

The fracture was found to commence in the right orbit, at the inner canthus,—ran directly up the os frontis, to the coronal suture, following then the sagittal suture about an inch, —thence obliquely across the anterior third of the right parietal bone, crossing the squamous suture near the middle of the temporal bone, inclining forward through that bone, it terminated just behind the external canthus. The deepest depression was at the junction of the sagittal with the coronal suture. A portion of bone was removed with the trephine just below and in front of this point. It was now ascertained that the serrated edge of the depressed bone, had lascerated the dura and pia mater, and had separated a considerable portion of the cerebral substance. The coagulated blood, and

detached brain, of the latter a table spoonful in quantity were removed. The depressed bone elevated,—the wound cleansed,—the parts approximated,—adhesive plaster and lint, with a roller, completed the dressing.

From the fracture's passing through the frontal sinus,—the great depression of bone,—and the fact, that the eyeball was thrown entirely from the socket, I am induced to believe the fracture extended some distance into the orbit. The patient having been placed in bed, I ordered warm frictions to the extremities,—a solution of sulphate of magnesia and tartarized antimony, in small and frequently repeated portions, together with stimulating enemas; with the view of establishing reaction, and counteracting the inflammation consequent upon so extensive an injury. I now left him, with directions that he should be bled, so soon as reaction should take place, provided the pulse became full and hard. The day after the operation I was taken ill, and was unable to visit him for four or five days, during which time he was visited once by a medical friend, (Dr. Taliaferro, of Washington, Kentucky,) who during my attack had visited me. He was also attended by Messrs. Rees and Woods, to whom, for their kindness and attention, both the patient and myself are much indebted.

On the 20th, I was able to visit my patient again,—found the wound beginning to suppurate,—tongue foul,—considerable delirium, and incoherent talking. Ordered 40 grains of calomel, followed by sulphate of magnesia, which brought off copious evacuations of dark bilious matter, with manifest relief. A considerable portion of the wound healed by the first intention, and its general appearance was favorable, until the eighth day after the operation, when an illconditioned abscess was discovered, just above the inner canthus of the eye, the consequence of a collection of matter in the frontal sinus, which, from the posture in which the patient lay upon his back, had been confined within the sinus, and became the source of much irritation.

About this time the febrile symptoms increased, he became restless, and complained much of his head and diseased eye,

which was still greatly swollen and prominent, not having entirely receded within its orbit. Vision in this eye, so far as we could judge, was very indistinct, if not entirely lost, at this period. The pupil seemed permanently dilated, the surrounding integuments very livid, and the vessels of the ball and tunics were in a highly congested state. For the removal of the pus in the frontal sinus, I made a puncture through the integuments, covering the inner angle of the eye and supercilliary ridge, so as to penetrate the fracture, and pass into the frontal sinus, at its most dependent point. The matter thus dislodged, by the aid of cathartics and diaphoretics, the febrile symptoms subsided. The serrated edges of the fractured bone, immediately over the sinus, became *carious*, and were carried off by absorption, which continued along the course of the fracture, as high up on the os frontis, as the edge of the hair, and terminated in the opening made by the trephine. After this there was a general but gradual amendment. Nothing worthy of observation supervened, until the closing of the external wound. The absorption of bone, and that removed by the trephine, presents the singular appearance of a deep figure, running in a perpendicular line upon the forehead, of two and a half inches in length, and half an inch in width, through the whole length of which every pulsation of the brain may be distinctly seen and felt.

The condition of the eye continued to improve, during the healing of the external wound. It was about twenty days from the time he received the injury, before objects could be distinguished with the injured eye. Every thing for several days appeared inverted—as vision improved, objects appeared more distinct, but double, and at a greater distance than was real. Whether these phenomena were the result of inflammation, and a congested state of the blood vessels of the eye, or from an elongation, or shortening of the axis of vision, consequent upon the protrusion of the eyeball from its orbit; from the dilated state of the pupil, or from the too great or diminished sensibility of the optic nerve and retina, or whatever else, I leave as subjects of speculation, to the ingenious anatomist and speculative physiologist.

A question of some practical importance, however, might here be asked, Whether the fortuitous occurrence of the abscess in the frontal sinus, and consequent long continued drain of pus, so contiguous to the injured eye, was not a very great means of finally restoring the eye, and establishing perfect vision? The time, from the injury until the entire wound healed, was about thirty-five days; the part over the sinus being the last point closed. Sight, however, was not perfect until near a month after the wound healed. All that was done for the patient after the abscess ceased to discharge pus, was to give him for a short time tonics, and keep a pledget of lint, wet with a solution of muriat. ammo. to his eyebrow and forehead. He now enjoys perfect sight, and use of his intellectual faculties. It is near three years since the operation; the boy has had good health up to the present time, with the exception of two attacks, which it may not be improper to mention in connection with the foregoing history of the case.

The circumstances connected with each attack I think will not be uninteresting to my readers, as they appear intimately connected with, if not consequent upon, the previous injury of the head. During the month of August, about seven months after the operation, having worked very hard for several weeks, at harvesting, and hauling stone for the foundation of a mill, he was attacked with severe pain of the head, with chills and fever. He was supposed by his father, a very intelligent old gentleman, to have an attack of bilious fever, for which he gave him a portion of calomel, and followed it with a dose of ol. ricini, about which time he had a convulsion fit, and continued to have strong convulsions every twenty or thirty minutes, for several hours. I was immediately sent for. On my arrival he had had sixteen fits — was in a lethargic state — cold extremities — pallid countenance, and feeble pulse. I applied warm frictions, sinapisms, etc. to his extremities, and believing, from what the family told me, that the cause of his attack was from some undigested substance in his stomach, which, from irritation, had affected the head sympathetically, I gave him a gentle emetic, which operated three or four

times, and brought off considerable food in an undigested state, and very sour. This was followed by a stimulating enema; after the operation of which the convulsions ceased, followed by intense arterial action. I now bled him freely, applied cold wet cloths to the head, kept up free catharsis by means of calomel and James' powder, followed by neutral salts. I left him much better; but in a few days, I was again sent for in haste, was informed by the messenger, "that the boy had been delirious all night, and talking incoherently, complained much of his head, that since daylight he had slept sound and could not be roused, that the family had discovered a large tumor or swelling on the forehead, over the open space occasioned by the loss of bone, and that the tumor could be seen to beat strongly." On my arrival I found the description given by the messenger, had not been exaggerated, but fell far short of the reality. He was catamose,—pupils dilated,—a tumor of the size of an egg, covering the artificial opening in the os frontis, and pulsating as strong and regular as the carotids themselves. A question would very naturally arise in the mind of the practitioner, Is it the cerebral substance protruding, or is it the collection of a fluid, and if a fluid, is it pus, blood, or serum? These are considerations which would produce some embarrassment in forming the diagnosis, as well as the proper course of treatment. From the symptoms, it was evident there was compression of the brain, from some cause. The tumor was firm, and regularly elastic. When firm pressure was made on it, it would recede within the cranium, but reappear immediately again, upon the pressure being taken off; and appeared to be attended with pain and restlessness. Upon a minute examination, I found the base of the tumor occupied a greater surface than the opening of the os frontis; the elasticity of its base being perceptible for some distance from the margin of the bone, surrounding the opening in the cranium. From these facts I was induced to believe that the tumor was produced by a fluid, which, insinuating itself between the integuments and bone, caused the fluctuation, which was perceptible at the base of the tumor, and larger in its circumference than the opening in the os frontis. Had

it been produced by a protrusion of a portion of the cerebral substance, its base would have been circumscribed by the margin of bone through which it passed, and would have been rather less than the opening in the bone.

Feeling confident the young man must die, unless the pressure on the brain could be taken off, I determined on making an opening into the tumor, by a puncture with the lancet, when, to the gratification of all present, it was followed by a free discharge of perfectly transparent serum. It ran in a full stream until we caught about six ounces, after which the water continued to ooze from the orifice for about thirty-six hours, when it closed. In a few hours after this evacuation of water, the patient was restored to his senses, and conva lesced rapidly. From the number of cloths wet by the fluid which oozed from the wound, it was believed by the family that the entire quantity discharged was not less than sixteen or eighteen ounces. The patient soon recovered, and continued to enjoy good health for eighteen months, when he was again seized with convulsions, accompanied with high febrile excitement, which had followed severe exercise. By prompt v. s., free catharsis, antimonials, and digitalis, the febrile symptoms were arrested, the convulsions subsided, and in a short time he was again restored to health.

In this last attack there was no tumor formed, except that during a convulsion there was a considerable prominence or convexity of the integuments covering the opening in the os frontis: affording conclusive evidence that, in convulsions, there is not only evident congestion of the brain, but actual enlargement of its volume, producing great irritability of the nervous system, with symptoms of compression of the brain; similar, though in a less degree, to those produced from a simple depression of bone; with this characteristic difference, that compression from depressed bone produces uniform and constant pressure, from the circumference to the centre; whereas compression from congestion of the vessels of the brain is from the centre to the circumference, and irregular in its application, from the convulsive systole and diastole of the heart and arteries; thus accounting, I think rationally, for the

effusion, in the first attack. The exertion in the hot sun, of lifting and hauling heavy stone, produced a determination of blood to the head, and consequent congestion of the vessels of the brain, followed by convulsions and vomiting. The soft parts thereby being frequently thrust backward and forward through the opening in the os frontis, became irritated and inflamed by the rough edges of the cranium, and the inflammation terminated in effusion; which termination was prevented in the second attack by early and decisive anti-phlogistic treatment then employed. Since his recovery from this last attack he has had no return of disease. His mental faculties appear as good as they ever were, and so far as we can judge, he suffers no other inconvenience from the accident, than that occasioned from the permanent loss of bone from the forehead. PHILIP J. BUCKNER.

Georgetown, Ohio, October, 1835.

ART. VI.—THE SICK STOMACH.

The "Sick Stomach," or "Milk Sickness," of the Western States, ascribed to the RHUS RADICANS *—by an unprofessional correspondent of* THE EDITOR.

In different volumes of this journal we have made publications, on one of the epidemic diseases of the west and south, known among the people and our country brethren, by the names prefixed to this article. It is not our intention at this time to give a full recapitulation of the symptoms of this malady, and of its pathological anatomy we know nothing. Vomiting, constipation, thirst, a white tongue, diminished secretion of bile, but little increased heat of the surfaces, and great muscular debility, are, according to the recollections of the moment, the prominent symptoms; all of which, when the disease assumes a chronic character, are increased by exercise. In its acute form, the patient often dies in two or

three days. Horses, cattle, dogs, and hogs, are thought, by the people where the complaint prevails, to be destroyed by it. In them, the prominent symptoms are muscular debility and tremens.

It appears that this malady was observed in some parts of North Carolina, more than sixty years ago; and that it has been an endemic in certain localities of Tennessee, Kentucky, and Ohio, from their first settlement—gradually, however, disappearing before the progress of cultivation. At present it is more prevalent in Indiana than either of the states just mentioned.

By some physicians, the "Sick Stomach" has with great confidence been ascribed to malaria; but, as it has prevailed equally in marshy, and dry elevated places, there seems to be but little to support this opinion. It has, also, been ascribed to a poisonous impregnation of the water of certain springs, but on a careful examination of some of these waters, we have not been able to detect any thing peculiar in them.

The favorite hypothesis of the people, has always been, that the herbiverous animals liable to the complaint, were poisoned by eating some kind of plant; and that the carniverous, as the dog and the hog, were affected by *eating the flesh* of such of the former as had fallen victims to the disease. Its production in man has, on the same theory, been explained by the use of the meat, milk, and butter of the cow, which had eaten of the supposed plant, without having been destroyed by it. The physiological difficulties which lie in the way of this hypothesis, have not of course been perceived by its authors, and *perhaps* are not insurmountable. They have differed widely, however, as to the particular plant, which occasions the malady; and we have at different times received a great variety of specimens, some of which were, from their botanical affinities, manifestly inert. The last specimen with which we have been favored, is from an intelligent and respectable unprofessional friend, Thos. S. Hinde, Esq., of Mount Carmel, Illinois; who has also lived in Ohio and Kentucky, and has seen much of the disease, in various places. The specimen he has sent us, is nothing more nor

less than the RHUS RADICANS—generally called poison vine. That this, like most of its congeners, is an active and noxious plant, there can be no doubt but after having spent most of our life in a region of country where it grows abundantly, we are compelled to say, that we have not throughout the whole period observed a single fact going to indicate, that this plant possesses the qualities ascribed to it. Nevertheless we shall extract from the communication accompanying the specimen, such paragraphs as will present the views of our correspondent:

"The milk sickness, or puking complaint, from the first settlement of the western country, has been a source of much affliction. In many parts of Kentucky and Ohio, and through the west generally, it has proved fatal to thousands, and this too, frequently under the most distressing circumstances. It sometimes appears by the agency of beef and butter, in market towns, and has there been often the cause of death, or much suffering.

"The two first persons whom I ever heard of dying with this complaint, were Mr. Bernard Fowler, and a Mr. William Tompkins, from Virginia, both near Bank-lick creek, on Licking river, in Kentucky, about the year 1795. Next I heard of the disease on Eagle creek, Raven creek, etc.; and when the Green river country was settled, it became prevalent there, as it likewise was in the neighborhood of the Red banks. All these places are in the state of Kentucky. In Ohio, on Paint creek, a branch of the Scioto river, especially the Rattlesnake fork of Paint, in the Mad river country, and on the heads of the Miamies, it prevailed at an early period, but was by no means limited to these localities.

"About the year 1807 I began to examine into the remote cause of this malady. The opinions of the people on this point were to me then as they still are, very conflicting. Being informed by an extensive stock farmer on Mad river, that he had followed his cattle into the woods and prairies, in the fall season, the period when they were most likely to take the *trembles*, he found them greedily devouring the leaves of a certain vine, and that they were soon afterwards taken ill,

with the well known symptoms. He gave each immediate relief, by the administration of a quart of whiskey.

"After having for more than twenty years been occasionally inquiring into the remote cause of this complaint, the suffering condition of some old acquaintances on Darby creek, in Ohio, fifteen out of two families having died of it, and my old friend having his *third* wife, and she the third wife having her *third* husband, I was induced to publish what these people had confirmed as to the cause, which gave rise to a series of publications in the Ohio State Journal, of Columbus, and the subject was warmly contested by an anonymous writer. The *truth* now is but too evident, wherever the vine referred to is found in any tolerable quantity, there this wretched complaint prevails to a greater or less degree. Some have given the vine to cattle to try its effects, and it has killed them; horses have eatenit, and it has killed them—cattle have been followed into the range, seen eating of it, and it has killed them; where two pastures have been separated by one fence only, one a woods pasture, and other meadow pasture, the cattle in the woods pasture have been found eating this vine, and many have died, on some occasions sixty out of a drove. One man tried an experiment upon himself, ate the leaves of the vine, and it produced, the next day the puking, with all the usual symptoms. A friend of mine in Ohio, Mr. John McNiel, of Oldtown, (now Frankfort.) tried the experiment on a steer which was in good health, and it died the next day. It is unnecessary for me to dwell on particulars: the vine is now well known pretty generally in the Mad river country, and the people avoid it."

The plant here designated as the cause of the "Milk Sickness," is, as we have said, the Rhus Radicans; but our correspondent indicates to us two others, which, he says, have been supposed to occasion the disease. They are the Bignonia Radicans, or trumpet flower, and the Vitis Hederaceæ, the latter a very common vine, having digital leaves, found along fences, and often seen running over old stumps. We are not aware, that either of these vines possesses deleterious properties.

The chief object of this article is to direct the attention of our western brethren to the causes and nature of the "Sick Stomach." It is certainly remarkable, that prevailing in so many localities, it should not have been better understood at this late period. Indeed, so defective are the accounts, which have hitherto been given of it, that many physicians have seriously doubted the existence of the malady, as an affection distinct from ordinary gastritis, depending on common causes. This is a point which a little diligence and perspicacity might settle.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Art. VII.—Materia Medica.

Materia Medica and Therapeutics, by Dr. A. G. Thompson. Second Edition, 1 *vol. pp.* 10–73.

(From a Correspondent.)

Professor Thompson, of the London University, is a man of extensive acquisitions in medical science. The work, an analysis of which we are about to present to the readers of this journal, is replete with instructive matter. Were our object a critical examination of the work, we could, like all other wise critical detectors of defects, discover much in its pages upon which our eager appetite for fault-finding might feed. There are errors, and serious ones, in this book, and were it not for its numerous excellencies, these errors would drag it down to rapid oblivion. But the inherent merits of the work will not only keep it from passing to the tomb of forgetfulness, but will urge its onward way to celebrity, and to a high appreciation of its numerous excellencies.

"The intention of the following pages," says Dr. Thompson in his preface to the first edition, "is to present a condensed view of the branch of medical science of which they treat. The extent and nature of the subject has been too little considered, and the preliminary acquisitions requisite for its proper acquirement most unaccountably overlooked in the course

of studies prescribed by the incorporated medical bodies in this country." The preliminary acquisitions here referred to by Dr. Thompson, are those of natural history, botany, chemistry, anatomy, and physiology, the pretermission of the study of which by the student of medicine is a just subject of reprehension. Yet there is another branch of preliminary study which should never be omitted by the student of our science —that of intellectual philosophy. And by intellectual philosophy we mean neither the erring fancies and flights of phrenology, nor the deceptious speculations of metaphysical subtlety, but a sound, authentic, enlightened view of the capacities of the mind, and of their appropriate methods of exercise in search of truth. There is a spirit or taste of the mind, sometimes the gift of nature, but oftener the result of a protracted discipline of its powers, which is far more valuable than all the limited acquirements of our youthful days; without which the noblest capabilities of genius are often lavished on topics of inquiry in themselves transcendental, or of very inferior consequence in science, but which a fond fancy clothes in the drapery of exaggerated importance. The history of materia medica bears emphatic testimony to the truth of the above remark, for during many ages it was little else than a history of vain attempts to discover universal specifics, and to substitute mere a priori reasoning for well ascertained facts. Even in this philosophic age of careful examination, of pains-taking experiment, of logical deduction, many errors are warmly cherished by physicians, on the virtues of medicinal substances. The laws of the animal economy are often lost sight of, in our speculations on the agency of remedies, and the materia medica seems reared up as a distinct branch of practical medicine, on ground alien from the territory of a just conception of the properties of life, and of their modes of action in health and disease.

But to proceed with our author. "Although the greatest portion of the work can be regarded only as a compilation, yet the author has introduced into its pages the results of 30 years of attentive and close observation in the treatment of diseases. With an enthusiastic love and a veneration for his

profession, which he is anxious to impress on those who are commencing the study of its principles, he has endeavored to trace the nature of the phenomena of morbid action, and to ascertain the actual influence exerted by remedial agents in effecting its removal." We have copied the above declarations of the author, because they reveal a proper and noble generosity in the mind of the writer, which should preposses us in his favor.

Dr. T. commences his work by a consideration of the general action of medicines, he then proceeds to a classification of medicinal agents, after which he discusses the properties of these agents, under their respective divisions of excitants, sedatives, refrigerants, etc., and closes by an appendix, containing tables of chemical equivalents, or atomic weights of substances belonging to the materia medica, and of ultimate components of several articles of the materia medica.

In part 1, section 1, we have some good reflections on the "general circumstances connected with the action of medical agents on the living body." "Medicines of an animal and a vegetable nature differ from food, in containing some active principle not adapted to repair the waste of the body; which resists the digestive powers, and often governs them: their other components, namely, resinous, albuminous, gelatinous, saccharine, oleaginous, feculaceous, and gummy matters undergo digestion.

"Medicines operate only on the living body: indeed, they may be regarded merely as means of affecting the vitality of the solid, whether that exhibit itself under the phenomena of *contractility*, *irritability*, or *sensibility*, or those that follow volition and association. To produce their effect they must be in immediate contact with some sensitive or irritable part of the body."

Dr. Thompson defines the term "medicine," a substance which is capable of altering the state of the body, whether in health or disease.

He contends that "there are five distinct modes in which medicines act upon the living body: they may act by a direct impression upon the surface to which they are applied, the

effect being confined to the part: 2, by an impression upon the nerves of the surface to which they are applied, and the effect to be extended to the other parts of the system; 3, they may be conveyed by absorption, undecomposed, into the system, and influence the habit through the medium of the circulation; 4, they may be decomposed, and operate only by one or more of their constituents; 5, they may operate by counter-irritation, or revulsion; 6, they may exert a chemical action on the tissues."

Medicines, agreeably to our author's views, act locally, when their influence is restricted to the part to which they are applied, as is the case of astringents, when given to check diarrhœa, or in the effect of collyria on an inflamed conjunctiva. The action of medicines on the nervous energy, the effect being general, is abundantly illustrated in the results of medication springing from the introduction of remedial agents into the stomach. "That every medicine which operates on the living solid exerts its influence, either directly or indirectly upon the nervous system, can scarcely be denied. Even when we admit that a medicine, which is absorbed and taken into the circulation, causes chemical changes in the circulating mass, still we must refer its ultimate effects to the nervous system, unless we suppose that the secretions are mere chemical changes in the fluids, altogether independent of the vital principle—an idea which is totally devoid of support." p. 5.

Even when a medicine is absorbed, according to Dr. T.'s views, it must be conveyed to a particular organ on which it is to act, and exert a direct agency on its living solid, which becomes "excited in the same direct manner as the surface to which the medicine was first applied; but at the same time, phenomena occur in distant organs can only be referred to nervous sympathy."

"Again," says he, "upon the whole there is abundant evidence to prove that many medicines produce their effects by not acting directly on the nervous energy independent of absorption. It is not essential that we should be able to demonstrate in what manner this communication with distant parts of the body is effected by the nerves. The attempt

to explain the phenomena is as vain as that respecting the vital principle; we find ourselves out of our depth, and the struggle only convinces us that we are in an element which does not belong to us."

There are three surfaces upon which medicinal impressions are made—the gastric and intestinal mucous tissue—the skin —and the organ of smelling. Dr. T. has omitted the mucous surface of the bronchii—an omission which is singularly unwise, for we have the most satisfactory proofs afforded in the mercurialism, induced by the vapors of quicksilver; in the rapid action of the nitrous oxide gas, when the nose is compressed in the act of inspiring it, and in other instances of foreign influences imparting their peculiar mode of modifying the vital properties of the system, through the pulmonary organs, that this channel of nervous susceptibility should never be overlooked in our inquiries respecting the agency of healthful, morbid, and medicinal impressions. It is true that we do not often apply our remedial agents to this point of nervous excitability, for, from the distinctive nature of the organic arrangements which obtain, in the lungs and of the special offices they subserve in the economy, a very limited opportunity is afforded for the administration of remedies through that medium. There are three modes in which a salubrious, or insalubrious atmosphere may affect the lungs. First, there is the essential action of oxygen in operating a change on the blood—secondly, there is the inspiriting power of a pure, free, well ventilated air on the nervous susceptibility of the pulmonary apparatus—and thirdly, there is a certain aerial temperature eminently conducive to their state of functional activity. A deprivation of oxygen leads to immediate death —a diminution of the proper quantity of it for the purposes of respiration, will, of course, induce a corresponding reduction of vital activity. Foreign matters in the atmosphere,—miasmata of various kinds—contagious effluvials affect the animal economy through the pulmonary mucous tissues. The pent up, stifling air, of a large city, although it may not be impregnated with paludal emanations, exerts a deteriorating influence upon the system—as soon as the fresh and free at-

mosphere of the country is breathed, the lungs dilate with renewed vigor, and the whole system feels refreshed and invigorated. The physical qualities of the air we respire exert a manifest agency on the lungs.

But to return to the text. Dr. Thompson is decidedly of opinion that medicines are absorbed in their entire state, and that this absorption may be effected either by the intestinal tube, by the skin, or through the lungs. Some medicinal substances are however decomposed by the stomach, after which gastric decomposition, they enter the system.

Our author makes some judicious remarks on the revulsive or counter-irritant action of medicines. Medicines operate often in this manner, and change the seat of morbid irritation, setting up a new centre of abnormal perceptions, in which an artificial and transient and manageable disease is substituted for one of a persistive and destructive character. Any potent medication exerted on the skin, alimentary canal, urinary apparatus, or salivary glands, may be effectively remedial in the cure of various morbid affections in organs whose sympathetic and functional acts are in consentient or antagonist relation to the surface, or structure influenced by our revulsions. Thus we blister to drive irritation from the stomach to the skin—we purge to create a revulsion of action from the head to the alimentary tube—we exhibit diuretics to produce a derivative action upon the kidneys, which shall subvert the hydropic effort going on in the capillaries of the subcutaneous cellular texture in anasarca.

"General effects of medicines on the vital solids and fluids, and on the functions;" under this head our author institutes an inquiry into the effects resulting from the action of remedies upon the living solids, and on the fluids, and on the functions of the principal organs.

The effects of medicines on the living solid are either stimulant or sedative—what the living solid implies is not easy to define—the term is employed to express an indefinite idea of what we suppose to be the ultimate fibril of the organic tissues;—every change induced by medicinal substances in the condition of any organ, results from an anterior modification of action wrought in this fibril.

Variations in the circulation may produce some difference in the state of the blood; "but this, if it be the effect of medicines, can be attributed only to their influence on the living solid, not to any chemical union or combination of the medicinal agent or its components with the principles of the blood. The blood may be mingled with medicines which have been taken into the stomach, and have passed into the circulation, either in whole or in part: but, during life, we see no reasons for believing that any material alteration, even of the physical properties of the blood, occurs from this cause. Some late experiments of Dr. Stevens, on the effect of saline substances upon the blood, seem to be at variance with this opinion: but these are too little understood to be brought forward as opposing the opinion which has been advanced." p. 19.

The general effects of medicines on the functions is evinced in the operation of many articles of the materia medica. Digitalis acts on the function of the heart, diminishing the pulsations of that organ; hyoscyamus and other narcotics abate nervous mobility—mercury increases the secretions of bile and saliva.

There is a just distinction which obtains between the physiological and the curative, or therapeutical effects of a medicine. The physiological action of a medicine depends upon the immutable and constant influence of the active principle of the substance operating on the organic tissues;—the therapeutical effects of a medicine are modified by various external and internal contingencies, which attach to the case for which it may be prescribed. Age, sex, idiosyncrasy, climate, and other contingent causes, besides diseases which exert the greatest amount of such modifying agency, these all, in a greater or less degree, exert a disturbing force on the constant regularity of medicinal agency. "Medicines exert no specific influence in curing disease, thence the terms febrifuge, antispasmodic, anti-scorbutic, and such like, are merely conventionals, to announce a secondary result consequent upon the appropriate action of some medicinal agent upon the vital solid." There are "many and very different circumstances which modify the curative operation of medicines." The chief modifying circumstances which exert a force upon the

action of remedial agents, are, original conformation of body, manifested in symmetrical peculiarity, or relative proportion as regards the head, extremities, and trunk of the body to one another;—in constitution, in idiosyncrasy, and in temperament; 2, age; 3, sex; 4, custom; 5, climate; 6, mental affections; 7, diseases, its stage, cause, seat, etc., and we might add in addition to these modifying influences, the effects of remedies which have preceded the medicines we administer.

Dr. Thompson, after copying the classifications of remedial articles given by Drs. Young and Murray, and urging objections to both of them, lays down his table of classification, which is the following:

I. Vital Agents.

A.—Influencing the body generally.

a.—By operating directly on the nervous system.

*Increasing action: Excitants.

**Diminishing action: Primarily, Sedatives, Refrigerants.

Secondarily, Narcotics, Antispasmodics.

b.—On the muscular and sanguiferous systems:

Tonics.
Astringents.

c.—On the secerning system:

Errhines.
Sialagogues.
Expectorants.
Emetics.
Cathartics.
Diuretics.
Emenagogues.
Diaphoretics.

B.—Influencing the body solely by their action on the part to which they are applied:

Epispastics.

a. Rubefacients.
b. Vesicants.
c. Erodents.

II. Chemical Agents.

A.—Influencing the body, or its contents, by their chemical properties:

*Acting on the surface: Escharotics, or Cauterants.

**Acting on contents of cavities: Antacids. Antalkalies. a. Antiseptics. Antilithics.

III. Mechanical Agents.

Demulcents.
Diluents.

Such is our author's classification, which, though we wish to abstain from criticism, we cannot refrain from the expression of our opinion, as radically erroneous. It is both too redundant, and too meagre. Redundant in the three-fold general divisions of vital, chemical, and mechanical agents—and meagre because he has omitted a very important class of medicines—alterants—from his table. We cannot enter upon this subject now, but will, in some future number of the journal, when we hope to show that a great error, in materia medica writers, has been in excluding alteratives entirely from a distinct consideration on their pages.

Excitants, agreeably to our author's classification, are the first class of medicines treated of. They may be defined—"substances that augment, powerfully, the motions peculiar to the different organs of the body, by a primary impulse on the sensibility and irritability of the part to which they are applied, communicated by the nerves to the whole system." Their general effects consist, 1, in an augmentation of nervous susceptibility—2, an increased action in the moving fibre—3, an acceleration of the frequency, and enhancement in the force of the pulse—4, a higher temperature of the body. There is a distinction between excitants and tonics. "Excitants increase the mobility of the system:—Tonics augment the strength of the muscles: Excitants exhaust the excitability; Tonics, within a certain limit, maintain it; the action of excitants is immediate, powerful, and transitory; that of tonics is slow, almost imperceptible, and progressive, but permanent." We need not give a table of excitants, they are familiar to every student. Dr. T. has placed iodine and mercury among the excitants. This is a bad collocation, for neither of them are stimulants, agreeably to his own definition. They are both alterants, and should have been placed under a distinct division. Strychnia, the active principle of nux vomica, is an excitant, which given in small doses increases the digestive energy, improving the appetite, and augmenting the powers of assimilation to such a degree as to increase the strength and size of the body in a rapid manner. If the dose is large, the respiration becomes oppressed, the respira-

tory muscles suffer a clonic contraction, and the person feels as if about to be suffocated. The circulation is not sensibly affected, and the urinary organs are little influenced, but the capillary vessels of the skin are powerfully excited, and copious perspirations occur during the action of strychnia. Dogs, cats, and rats, are affected by strychnia, or nux vomica, with symptoms of tetanus, distention of the limbs, tremors, immobility of the eyes, etc. Hogs and goats eat nux vomica with impunity. M. Desportes found that it produces very little effect upon poultry. Tincture of iodine is an antidote to strychnia. Strychnia produces no effect on the system when the spinal marrow has been previously destroyed. It acts, therefore, on the motor set of nerves, the anterior nerves of the spinal column. Strychnia has proved useful in pyrosis, chronic diarrhœa, and dysentery. Dr. Beardsley has found it efficacious in amenorrhœa. The extract of nux vomica, as well as the strychnia, have been of late much employed in paraplegia and other forms of palsy. The dose of strychnia is from one-twelfth to one-fourth of a grain—that of nux vomica half a grain to two grains. Neither of these active articles should be administered where there exists any symptoms indicative of inflammation in the brain in paralytic affections. The author enters into rather an uncalled for explication of the best modes of accumulating and administering electricity for medical purposes. Fourteen pages are thus occupied. Twelve pages are appropriated to caloric, as an excitant—and much of the matter on these twelve pages are irrelevant to the strict points of materia medica.

Iodine is employed as a general excitant, both in its uncombined and its combined state. Simple iodine may be prescribed in the form of pills, or in tincture. M. Luyal prefers the aqueous solution. There are several combinations of iodine with other substances—such as the ioduret of sulphur, ioduret of potassium, (hydriodate of potassa,) ioduret of iron, ioduret of mercury, ioduret of lead, and ioduret of arsenic. As a local application, the ioduret of potash is employed by friction over scrofulous tumors.

Iodine and its preparations stimulate in a special manner the capillary system. As a therapeutical agent, iodine has proved successful in bronchocele, and in scrofulous affections. In that form of scrofula which attacks the mesenteric glands, its employment is of little avail.

Mercury was considered by the ancients as a poison. In its metallic form it exerts no influence on the animal frame; "combined with sulphur, iodine, chlorine, cyanogen, oxygen, and acids, it enters the circulation, exciting powerfully the whole of the glandular system, and increasing in a remarkable degree both the secretions and excretions." We believe in neither of these positions of the author. Mercury, agreeably to our conceptions of its modus agendi, neither enters the blood, nor is it in a strict and accurate sense a stimulant. On these two points we cannot forbear copying a page or two from an essay lately published by us on the subject, entitled "Diseases induced by Mercury."

"There are several very important respects in which, if mercury be a stimulant, it differs from all other articles of that class of remedies. First, other stimulants are withheld by the enlightened practitioner in conducting the treatment of diseases of acknowledged inflammatory character. Take, as examples, dysentery, acute hepatitis, iritis, puerperal peritonitis; to administer brandy, camphor, or any other stimulant, for the cure of these diseases, would be a strong evidence of dementation. And yet mercury is the sheet anchor of the treatment in such diseases. In the second place mercury refuses to act when the system is too much prostrated in its powers. But stimulants are always given when the vital energies are sunk for the purpose of increasing them. * * * * To secure and perpetuate the train of action created by stimulants, we have to increase the quantity administered, and often to change the agent, because the impression induced by them on the organic sensibility is constantly diminishing. It is otherwise with mercurial excitement: to continue the remedy in a less dose than the one commenced with, after the process of mercurialism is instituted, is to greatly enhance the effect. The period which sometimes elapses between the ad-

ministration of mercury and the development of the salivant effects of the remedy is often considerable. In this a wide difference is seen between its action and that of any agent belonging to the class of stimulants. Swediaur, in his work on Venereal, mentions a case in which the mercurial action did not break forth till more than a month after the use of the mineral! * * * John Hunter, Philip Syng Physick, Bostock, Christison, with many of the other acute and herculean minds of the profession in modern days, have never been able to detect mercury in the blood, saliva, and bones, in the manner *it is said*, the wonder-loving explorers in this dark mine of inquiry, in former ages, found so readily and in such abundance. * * * If mercury enters the blood in one patient, it should do so in every one, to whom it is given to an extent sufficient to salivate. If it does so in every case, why did not Physick, Seyhert, Bostock, Christison, Mayer, Devergie, and many other acute experimental inquirers detect it, assisted, as they were, by the lights of modern chemistry?"

But to return to the work before us. Dr. T. minutely notices the various pharmaceutical preparations of mercury. These are to be found in the common works on materia medica, and in the Dispensatories.

"It is curious to observe," says he, "the extraordinary revolution which has taken place, at various periods, with regard to calomel. The ordinary dose, at present, when we are desirous of bringing the system under the mercurial influence, is from one grain to two grains, combined with opium, taken every night, or every morning or evening, or, at the utmost, three times in twenty-four hours, until the gums be affected. Schrœder relates that, in his time, the dose was half a drachm; Geoffrey makes it from six grains to thirty; Neuterus gave, at first, fifteen grains, for the second dose a scruple, for the third half a drachm, and for the fourth a scruple, which he continued till salivation was induced; and Michaelis Albertus informs us that Helwichius gave five scruples for a dose to two patients, and seventy-two grains to a third, which affected the mouth for a fortnight. Even when it was not intended to affect the mouth, scruple and half drachm doses were very

common in the seventeenth century; and at this day in India, scruple doses are usually prescribed in the bilious remittents of that climate." p. 275.

Among many excellent remarks and suggestions in reference to the employment of mercury, Dr. Thompson has made some very superficial and erroneous reflections. Thus, he recommends that calomel should be rubbed on the gums, to produce salivation, and credulously adheres to the old notion, contrary to the best modern lights, that metallic mercury can be found in the bones of persons who have been ptytalized.

Syphilis, he contends, is best treated by mild mercurials, pushed to a very gentle salivation.

Under the class of excitants, we have joy and impetuosity —but these remedies are not of easy administration—for "therein must the patient minister unto himself."

The therapeutical employment of excitants is rather rapidly dwelt upon—and in his remarks on that point we see nothing worth transcribing.

"Sedatives—Medicamenta Sedantia.—Sedatives are substances which directly depress the energy of the nervous system, diminishing motion in animal bodies without previous excitement. Regarded in a remedial point of view, they are powers intended to diminish preternaturally increased action in animal bodies. * * * In general, writers on the materia medica have confounded sedatives with narcotics; from which however they are perfectly distinct." Sedatives act on the nerves of a part to which they are applied, by a diminution of their capacity to feel impressions, and upon the irritability of the moving fibre, by lessening its energy. When introduced into the stomach, they induce a sedation of its functional activity, and through the gastric nerves influence the heart, brain, etc.

Sedatives, from the nature of their effects, may be arranged under two distinct heads:—direct sedatives, or those which operate immediately on the nerves; and indirect sedatives, or those which operate through the medium of the vascular system.

The direct sedatives are cyanogen combined with hydro-

gen, constituting hydrocyanic acid, laurel water, and volatile oil of almonds, or combined with potassium, in cyanide of potassium. Empyreumatic volatile oil,—in tobacco smoke: nicotiana, contained in the leaves of nicotiana *Tabacum*; sulphur, combined with hydrogen, in sulphuretted hydrogen gas, and hydrosulphuret of ammonia: and carbon, combined with hydrogen, in carburetted hydrogen gas.

The indirect sedatives are carbon, combined with oxygen, in carbonic acid gas, and bloodletting.

On the therapeutical employment of hydrocyanic acid and its compounds the author dwells with some particularity. In no kind of idiopathic fever has hydrocyanic acid been employed. In some of the phlegmasiæ it has been found beneficial, especially in phrenitis and pleurisy. As an external application it has proved useful in impetigo; in combination with acetate of lead, in the proportion of f℥ij. of the hydrocyanic acid, sixteen grains of acetate of lead, half a fluid oz. of alcohol, and f℥viij. of *distilled* water. In *prurigo*, *inveterate psoriasis*, and several other cutaneous affection, attended with severe itching and tingling, it has rarely failed in affording relief. In active hemorrhagic affections, experience authorizes the author to place much confidence in this acid. In cases of this character he has augmented the dose from m. iij. to m. xij., or until the pulse gave indications of the power of medicine, and in but few instances, where there was not some serious organic disease, have his expectations been disappointed.

In phthisis its powers have been greatly overrated—it is of less utility as a palliative in this mortal malady than opium. In the last stages of consumption it hastens the final catastrophe, by increasing the patient's debility. It is almost a specific in that affection of the trachea which has been named phthisis trachealis; and which, if the career of inflammatory action in the mucous membrane be not checked, is as fatal as tubercular phthisis itself. In chronic catarrh it is often efficacious. In dysentery it is of great service, given to the extent of four minims, or drops, for a dose, with calomel. Dr. Elliotson has published some cases of dyspepsia, in which the

hydrocyanic acid was of signal efficacy. Three drops with 3i. of the tincture of calumba in cold water, Dr. T. recommends as the best mode of exhibition. In pyrosis and asthma it is of great benefit. In hooping-cough it is to be regarded "as the sheet-anchor of the practitioner;" few cases of this disease, our author thinks, would prove fatal, were the hydrocyanic acid early resorted to and judiciously administered. "After emptying the stomach with an emetic, and purging briskly, the use of the acid should be begun, and the prescription never altered, except to increase the dose. When thus treated, the disease seldom continues more than a month or five weeks, particularly if the little patients be confined to a graduated temperature, and their diet be solely milk and vegetable." p. 325.

"It has been proposed to employ the essential oil of bitter almonds, instead of the free hydrocyanic acid; on the plea that the strength of the medicine can be more certainly determined; that it is less likely to suffer change from air, light variations of temperature, and age. Twelve drops of the volatile oil of the bitter almond may be regarded as equivalent to m. iv. of the ordinary medicinal acid. One objection, however, to its use, is the powerful effect which it has on individuals of a peculiar idiosyncrasy; namely, that of producing a rash, resembling nettle rash. In some people, eating a single bitter almond will produce nettle rash, vomiting, and vertigo; and it is probable that this depends on something in the fruit, independent of the hydrocyanic acid; as instances are met with in which the sweet almond causes the same inconveniences, nevertheless no hydrocyanic acid has ever been found in it."

"I once ordered it as an ointment, largely diluted with spermaceti ointment; but the effects were such as to prevent me from again prescribing it; and Coullon has recorded the case of a child who was killed by the leaves of the prunus laurocerasus applied to a sore on the neck." p. 326.

We will pass over his remarks on the other sedatives till he reaches bloodletting. There are some very important practical observations under that head. "During life the

bloodvessels are always in a certain degree of tension, by which the tone of the system is maintained; bloodletting diminishes this tension, and is followed by a relaxation and debility; consequently this operation produced a sedative effect on the frame."

The circumstances which modify the result of bloodletting are — 1, the mode of performing the operation, whether by opening a vein or artery; by general or topical methods; by a small or large orifice — 2, age — 3, temperament—4, sex — 5, mode of life—6, climate—7, quantity abstracted; and, we might add, the nature, stage, etc. of the disease. Very young and aged persons bear bloodletting worse than those in the middle periods of life. Men tolerate bleeding better than women—and agricultural people will sustain the loss of blood better than inhabitants of a city. Persons of sanguine temperament, our author thinks, bear bloodletting worse than those of a bilious or phlegmatic temperament. We are of a totally opposite opinion. Venesection is, ceteris paribus, better borne by dwellers in a temperate climate than those of tropical or hyperborean regions.

The quantity of blood drawn has its effects materially modified by the size of the orifice through which it may be detracted. A smaller amount of blood, lost in a rapid manner more deeply affects the circulation, than a larger slowly drained off. Dr. T. controverts Dr. Marshall Hall's position respecting the indispensableness of inducing syncope in cases of severe inflammatory attack. We are not always to wait for syncope, for this is not absolutely essential to secure the beneficial sedative influence of bloodletting. Neither will the rapid superinduction of deliquium animi by the use of the lancet afford any sure evidence that the inflammatory action has been arrested. Cupping, for rapid extraction of blood from a part, is preferable to leeching.

"Practical Employment of Sedatives.—All the sedatives which we have examined, with the exception of bloodletting, are powerful poisons; nevertheless, when they are properly administered, andtheir effects care fully watched, they are possessed of powers that cannot be obtained from any other

medicines. They are chiefly indicated in diseases of increased sensibility and irritability. As the remedial effects of most of the substances described have been already mentioned, I will confine my remarks chiefly to bloodletting. In intermittent fevers, sedatives are not indicated, unless we except bloodletting, which was employed as the sole means of cure in these diseases by the older physicians. The practice partly arose from the mistaken notion that a morbific matter existed in the blood, and required to be evacuated—an opinion now justly exploded."

"Whilst the cold or sweating stage is present, it should never be resorted to; for in the first the vital powers are depressed, and in the second they are exhausted." The symptoms during the interval indicating the loss of blood are, fulness and tenseness of pulse, headache, throbbing at the temples, and flushed face.

In synocha, or open inflammatory fever, bloodletting is of great moment, and should be rigorously employed. In large towns fevers require less the use of the lancet than those occurring in the country; in damp situations than in dry; and in crowded hospitals than in private practice.

In typhus, bloodletting employed at the commencement of the disease, will accomplish good results.

In plegmasia, bloodletting constitutes the main curative agent. When, however, the protecting influence of the local inflammation is withdrawn, or greatly abated, then the hand of vascular depletion must be stayed.

In opthalmia, croup, pneumonia, pleurisy, and in the hemorrhages accompanied with general momentum of circulation, the necessity of venesection is obvious.

"In phthisis, bloodletting has been much employed. In the inflammatory stage of the disease, bloodletting is our principal remedial agent. It must be early employed; the object being to diminish vascular action; but in this deplorable malady the symptoms are not always so obvious as to lead to decisive measures; the inflammatory action steals on insidiously, and the disease, when it is only expected, has already established itself in the frame. Still, if suppuration have not taken place,

the lancet may prove useful; but the utmost discrimination is required in directing its employment. In the second stage of the complaint, when tubercles are in a suppurating state, much relief has been afforded by small and frequently repeated venesections; the quantity taken at a time should not exceed a few ounces; seldom more than three. Phthisical patients, even after hectic shows itself, bear such bleedings well; and instances are recorded in which they have been bled two hundred times in a few months." p. 350.

In dysentery, contrary to our experience, Dr. T. does not employ the lancet.

"Spasmodic and convulsive diseases depend on increased irritability, and, consequently, whatever favors debility is to be avoided. They are sometimes, however, complicated with an inflammatory diathesis: in which case blocdletting is not to be neglected. When there is a determination to particular parts, although the phlogistic diathesis be not present, cupping and leeches may be proper.

"In apoplexy, the lancet is resorted to during the attack, and also in the interval to prevent the recurrence of the attack, from the idea of congestion or effusion of blood within the cranium: but this is not always the case; and apoplexy may proceed from causes depressing the powers of life, without the presence of actual plethora. When a state of plethora really exists, with an evident determination to the head, bloodletting cannot be dispensed with.

"In mania, the general use of the lancet has produced much mischief. It is true mania is accompanied with the most violent excitement: but in no case is this state so rapidly followed by collapse. How far the employment of other sedatives may be safely proceeded with in these cases is still a question for experience to decide."

"Refrigerants—'Medicamenta Refrigerantia.'—Under this head the author indulges in quite an idle display of speculative ingenuity—which we shall pretermit. His table of refrigerants is as follows:

"Refrigerants operating on the organic functions, are the vegetable acids—nitrate of potash, and biborate of soda:—

those operating on the sensibility are, cool air, ice, cold water and evaporating lotions." The benefits resulting in various diseases from cool air, cold water, ice, and evaporating lotions need no special enumeration at our hands. The use of these measures is so familiar to all engaged in the practice of the profession, that we shall say nothing about them.

J. P. H.

(To be continued.)

Art. VIII.—Medical Experience.

Essays and Lectures on Medical subjects by John P. Harrison, M. D. Professor of Materia Medica in Cincinnati College—Philadelphia—J. Crissey 4 Minor-street, 1835.

This small work, seems to be especially dedicated to students of medicine; and unquestionably the subjects discussed in it, from their own intrinsic importance, well merit the serious consideration of those, for whom the book is designed. Yet, we would by no means be disposed to limit its circulation to students of medicine alone; for there is much contained in it, worthy the attention of the older members of the profession. The essay which first claims our notice, not only from its locality, but from the importance which the author himself seems to have attached to the subject, is on *medical experience* and occupies about one third the entire book. In this essay the author contemplates medicine in two aspects, as an art and as a science. He speaks of fallacies peculiar to each, but indicates strongly that the fallacy of "too much learning" is much the most dangerous, from it being often times shrouded in the splendors of "literary accomplishment." We know the proneness of human nature to love the mysterious, and to be captivated by the wonderful, and we feel assured that it is upon this wonder-loving principle with which humanity is so largely imbued, that the empiric in medicine is indebted for his success.

Although this is avowedly the age of enlightenment—although the progress of human knowledge has been such as to astonish even those who have kept pace with its improvement,—notwithstanding the arts and sciences have attained a degree of perfection, of wh'c , the bare prediction but a few years ago, would have subjected the unhappy individual to the charge of lunacy; and our beautiful science, too ahcieving triumph after triumph, yet all have been unsuccessful in liberating the mind from the shackles of a delusion that has entwined it for centuries. Our author says:

"Though delusion after delusion be exposed and perish, like images traced in sand by the sea-shore, which the next wave obliterates, yet the spirit of delusion, the love of deception, perish not, become not weary with the progress of years, with the experience of time. One form of error rises, is received with favor, eulogized as the messenger of great good to man, endures for a season, tili another one arises to supplant its older rival, and like it, flourish in brief existence, then fades away and is forgotten.

"The science, as well as art of medicine, to the eye of the philosophical beholder, resembles Virgil's mystical tree at the mouth of Avernus, thickly covered with visionary leaves, and on every leaf a dream. But it is a great and useful art, a noble and elevated science, well deserving of the highest appreciation, and most exalted esteem. For however obscured by error, and perverted by quackery, the art of healing is the source of many blessings to man. And as a science, medicine is of liberal bearing, and full of intellectual greatness; having for its object the expansion of the human mind on some of the most interesting topics that can invite investigation. Man, in his physical frame, in the organic structure, and physiological laws of that frame,—in his capacities of thought and emotion, and the inscrutable connexion of these divine capacities with his physical frame,—in the diseases so various and numerous, which afflict his body and mind,—the remedies which a sound experience has authenticated as most proper to remove those diseases,—and the collateral and auxiliary sciences, which pour in their contributions of aid, of light of power, into the storehouse of this great and noble science.—All these attest the dignity, and effectively proclaim the high and useful bearing of medicine on the well-being of mankind."

We do not claim for the author exclusive originality; but we say, he has set forth his views in an intrepid and manly way, and the general sentiment which pervades the essay, is such as to do honor alike to his head and heart. We are by no means either disposed to say that error does not exist, since this is incident to every human performance, and honesty obliges us whilst we award the meed of praise to that which

is excellent in its kind, not to neglect or pass over that which may be error, and to our mind is error.

The author has remarked that language is the "interpreter of thought," and laments it as most unfortunate that it has been so frequently perverted from this its high function.—Indeed he remarks it is even more than the interpreter of thought. "It is embodied mind; it is tangible ideas; it is intellect made visible." Here we apprehend, we might with propriety take up the lamentation of the author and ask that all "ambiguity and imprecision be banished from the language of every cultivated man, who wishes to impart his thoughts with force, perspicuity and certainty." See page 13.

Medicine he contemplates as a physical science, and justly remarks, that it owes many of its proudest triumphs to the inductive philosophy—skill in the application of remedies, unwearied observation of the phenomena of their operation, and correct deduction, are those means which have tended most to the advancement of our science. It is a point conceded, that no science can be correctly taught or its principles explained, if the language used for such teaching or explanation, be ambiguous or indefinite. Hence our author has properly said, that "the terms of medicine should be well defined;" the language of physicians so definite as to convey unerringly "the precise ideas existing in their minds," and the nomenclature of our science divested of all ambiguity of expression, so that the human mind may be no longer retarded in its efforts after new discoveries, with which to adorn and ennoble our science.

"The science of medicine owes much to experience, but it is an experience at once discriminative and profound. The collected light of ages beams around her path. To the assiduous and enlightened cultivators of our art from the days of Hippocrates to the present age of the world, we are indebted for that fund of facts and rational deductions which go to constitute the science of curing diseases. No one individual, in this age, should arrogate to himself an original experience completely independent of the past experience of the medical world. Indeed, the most skilful in our art pretend to nothing more than a judicious combination of the experience of the past with their own observations—an application of that knowledge derived from erudition with that acquired by personal inspection, and a careful analysis of the opinions of their predecessors, so as to build upon it a manly and self-sustained judgment, which shall enable them to disentangle the per-

plexities of disease, and to send a discriminating eye over all those details of medical practice which are so apt to embarrass and confuse weak minds.

"Each physician must go through a discipline in the school of knowledge; each one has to advance from ignorance to wisdom by slow and measured steps; no man can, per saltum, reach the pinnacle of medical science; he might as well attempt to construct a dome without a foundation. All valuable acquisition is the fruit of toil."

In the further prosecution of this subject the author attempts to show "the sources of false medical experience, and in the next place proceeds to notice the best method of acquiring a true medical experience."

I. *False medical experience* he subdivides as follows: 1. Superstition, 2. Credulity, 3. Fallacy of testimony, 4. Ambiguity of language, 5. False theory.

It is not our purpose to go into a minute analysis of the author's remarks, upon each of these separate subdivisions; but simply to extract from each that which will convey a general impression of the whole.

1. *Superstition.*

"The three most intense of human predilections, considering man as an intellectual, social, and physical being, are the main avenues through which tyranny has made its most successful approaches, in rude states of society. These are, the strong desire man feels for the preservation of life, the irrepressible aspirings of his nature towards religious emotion, and his earnest want for some kind of polity. In ignorant and rude conditions of society, there is often a union of the functions of the physician, priest, and chief, in one individual. We need not, however, draw our illustrations in the present instance from barbarous tribes, for the celebrated Prince Hohenlohe, perhaps still alive, will furnish a most apposite example of such a species of folly.

"Alexander Leopold Hohenlohe, a Hungarian prince, whose father was disqualified for government by mental derangement, and whose mother was of an ardent temperament, and of fervid zeal for her religion, began in 1815 to manifest his talents for preaching agreeably to his fond mother's hopes and entreaties. 'After having received the papal permission to consecrate three thousand rosaries, crucifixes, &c. at once, he left Rome and went to Germany, where he was considered by his colleagues as devoted to Jesuitism, and an enemy to knowledge.' Martin Michel, in Baden, a worker of miraculous cures, through the power of faith, and the efficacy of prayer, assured the prince, that the faith and prayers of a prince, were of much greater prevalence and potency to effect miraculous cures than were those of a common peasant, such as Michel was. The imagination of the fanatic prince was fired at once, and he soon proceeded to put into execution Michel's plan of healing the halt, the blind, the impotent, and the insane. A princess, Matilda, of Schwartzenburg, who had distortion of the spine, was made to walk

through the agency of the princely priest and doctor, aided by Michel, his guide, encourager, and friend. The prince soon, however, commenced operations on his own responsibility and prayers, and multitudes eagerly crowded around his path. This spiritual physician's fame soon diffused itself over Germany, the country of the ideal, and reached even England and America. His enthusiastic admirers published many, in their opinion at least, veritable cases of terrible disease, which they solemnly averred were cured by the great apostle of delusion. He could not succeed in his cases in the hospitals of Wurtzburg and Bamberg, and a prince of Hiedburghaosen, who labored under an affection of the eyes, in consequence of discontinuing all medical applications, and relying on this spiritual doctor, found his eyes soon get much worse. Still the confidence of Hohenlohe was so great, and his presumption so overweening, that in 1821, he solicited the Pope's attention to his miraculous endowment, but Pius VII. intimated his will to be, that the process should not be designated a miracle, but *priestly prayer for healing.*

"After Hohenlohe's failure upon the prince, with diseased eyes, he declared himself exhausted, and was unwilling to be seen by the health police whilst in the act of performing his cures. Of late, he works at a distance, and the newspapers, and some of the medical periodicals, have given us accounts of the reputed miracles performed by his highness, or excellency, in France, England, Scotland, Ireland, and the United States. His mode of late, is to direct the afflicted person, making application for cure to him, to pray at certain times of the day, cotemporaneously with himself. It has been objected to these simultaneous prayers by some caviling spirits, that a prayer at 8 o'clock, in Hungary, has been ended before that of 8 o'clock, at Marseilles, begins; but to this objection, urged by the sceptic, one irrefutable reply is enough, it is all miracle, or all superstition—from alpha to omega—and admits no investigation, but demands a simple, unquestioning faith, too facile for doubt, and too submissive for reply.*

"The above furnishes a melancholy proof of the perverted application of that scheme of human illumination, which Christianity furnishes, and is a signal verification of a remark already made, that mankind love the luxury of delusion, and seek its gratification in the most exorbitant exhibitions of folly."

2. *Credulity.*

This vice, the author contemplates as near akin to superstition; and certainly, since they both emanate from one prolific parent, *ignorance*, their relationship will not be doubted. When this passion predominates in the human mind, there is no proposition, either in Christian ethics or in medicine, too absurd or preposterous not to receive full credence. It is not asked that it should be sustained by facts—that experience and observation should be the touchstone by which its truths might be demonstrated. The author proceeds to say:

* Encyclop. Americana: article, Hohenlohe.

"A warm and animated zeal for any cause, unless that zeal is chastened and regulated by a severe discipline of judgment, is sure to mislead the mind. Quod volumus, facile credimus, is as true in medicine as in the ordinary pursuits of life. Our science is filled with convincing illustrations of this position. The discoverer of every new remedy asserts that he finds in it an epitome of the whole *materia medica.* The metallic tractors of Perkins, the animal magnetism of Mesmer, the elixir of life of Paracelsus, the alkahest of Van Helmont, and the panaceas and catholicons of the present day, all have derived their source from fraud operating on credulity. Even where no sordid considerations influence the propagator of a revived, or newly discovered remedy, the ardent enthusiasm of the experimenter urges him to great extravagance in the commendations he bestows on it. Baron Storck greatly overrated the remedial power of the Narcotics, upon which he made so many experiments. There is no prominent remedy in our catalogue of agents for the removal of disease, but what has been at one time surrounded by a mist of panegyric. And how many inert substances have been eulogized, that are now quietly slumbering in the tomb of forgetfulness! Opium, digitalis, mercury, antimony, and the bark are the most effective weapons of medicine. Each of these have been injured in their legitimate character, as medicinal substances, by the indiscriminate encomiums lavished upon them. The most enlightened physicians now agree with the illustrious Sydenham, 'that the practice of physic chiefly consists in being able to discover the true curative indications, and not medicines to answer them; and those that have overlooked this point have taught empirics to imitate physicians.'"

Again our author remarks:

"Under the head of credulity, most appropriately, perhaps, are to be included the reveries and figments of Hahnemann's subtle but prolific brain. Endowed to a degree far beyond the common rate of even Germanic ideality and metaphysical vapouring, the author of the system of Homæopathy, has challenged human credulity to the utmost, and put to test the strength of man's addictedness to abandon his mind to the wildest wanderings of hypothesis: if the hypothesis is enforced by confident appeals to testimony, and a determined hostility towards all established views. Mixing up with the crudest modes of explaining the action of remedies, much parade of learning and professions of philanthropy, the abettors of this system, most emphatically denounce and villify all the generally established modes of considering, and of treating, disease. The captivating blandishments of infallibility and impeccability are, of course, the supreme features of homæopathism. It is the wisest, safest, best, and most sure mode of treating all human maladies. All other plans of conducting the treatment of disease are, in comparison to the homæopathic, but mere clumsy artifices of medical skill, mere potent engines of perpetuating morbid action, and of perpetrating death. Hahnemann, like another Bacon, has promulgated his organon, the leading doctrinal peculiarity of which is, *similia, similibus curantur*, in accordance with which to cure a disease, you must administer medicines that will excite symptoms similar to the disease—and thence is derived the name of the system, homæopathy, or as it is sometimes written, homoopathy—from the Greek words, *omoion* alike, and *pathos* disease."

3. *Fallacy of Testimony.*

In the long list of agents, which have exerted a potent influence in retarding the advancement of medical science, probably none outranks the fallacy of medical testimony.—False facts have been the bane of medical improvement, and as the author has truly remarked, the materia medica has in an especial manner been subjected to the ruinous and blighting effects of false experience. The cause of this however is obvious. Intimately associated with the remedy is the disease for which it is prescribed—hence the deductions of cure from the application or use of a particular plant, have been made upon insufficient data, and without that mature reflection and deliberation, which should at all times characterise the speculations of the philosophic and intelligent physician; or it has been done for the sordid purpose of making money, even at the expense of human life.

In illustration of this division of the subject the author instances among many others the case of Stone, said to have been cured by the celebrated Mrs. Stephens, whom the British Parliament so liberally compensated, after the individual was pronounced cured by several of the most enlightened medical men of that day. But this man was not cured—the stone could not be detected by the sound, because it was lodged in a pocket formed probably by a fold of the bladder. An interesting case of this kind, is reported by Professor Jameson in the present number of the Journal; in which by the most patient and laborious investigation and research, he was for a long time unable to detect the calculus, and yet the man had every symptom of the disease. The pocket in which this lodgment of calculi took place, was not discovered till the finger was introduced into the bladder, through the opening made for the extraction of the stone. See page 216.

"It were a task of immense labor to advert, by special enumeration, to all the numerous cases of such fallacy, as that arising from extravagant views of the medical properties of the various agents, which from age to age, and from year to year have received the unsparing and undistinguishing eulogies of physicians. And not contented to expatiate on the virtues of medicines which the ingenuous dealer in drugs kept, appropriately labeled, in his shop, the most accomplished physicians have at times departed from the employment of the remedies known to the profession, or recommended by men of scientific candor,

and too enthusiastically attached themselves to the use, and too eagerly recommended remedial agents, whose composition was a secret, and the authors and vendors of which, knew nothing of the just principles of medical science.

"The splendor which at one time surrounded the reputation of Swaim's Panacea, was derived from the dazzling brilliancy of that light which several of the luminaries of our American medicine threw around it. With precipitate and onward haste, some of our most enlightened physicians were found swelling the loud chorus of praise which was sounding forth the many virtues and transcendant excellencies of this nostrum. Certificates, signed by several of the most eminent medical men of the country, quickly found their way into the newspapers, and were by them rapidly disseminated through the Union."

Here we have an objection to the manner in which the author has attempted to gloss over the conduct of "certain luminaries of American medicine" (as he terms them) in certifying to the virtues and efficacy of Swaim's Panacea. Such conduct would be reprehensible in the humbler members of the profession; but when men occupying the most dignified and elevated places in medicine, prostrate the nobler purposes of their profession, to pander to the vile cravings of a sordid *empiric*, the scorpion lash of an indignant and dishonored profession should be exerted to correct the abuse and not tamely rest satisfied with their simple acknowledgement that they had "overrated the value of the Panacea of Swaim, and have for a long time ceased to prescribe it." Why ever prescribe it? Were they not emphatically acting the part of the *charlatan* in prescribing a panacea the constituents of which they knew not? We should like indeed to see the difference pointed out between the *nostrum vender* and these *certifiers* to its virtues. If there is any, the advantage is on the side of Swaim, who knows nothing, neither pretends to know any thing, of the exalted principles of medical science; whilst these, are proud and distinguished teachers in medicine.

We would here in conclusion express the hope that they have read with careful attention the report of the Medical Society of Philadelphia, drawn up by Horner, Harris, Klapp Meigs and Bell, and have resolved for the future, that they will not encourage the dissemination of nostrums by the influence of their names even though great *pecuniary loss* should be sustained.

4. *Ambiguity of Language.*

We agree with the general tenor of the author's remarks under this head and assume the position as incontrovertible that accuracy of language is the only sure method of making ourselves perfectly intelligible to those whom we are addressing

5. *False Theory.*

It does seem to us that the author under this head is liable to the charge of endeavoring to establish a theory if not false at least unsupported by that mass of testimony and well ascertained fact which are adduced to sustain the doctrine of the humoral pathology as taught and believed at the *present time.* He does not treat the doctrine with candor—he does not give it that consideration to which it is entitled—he speaks of the old exploded views of the humoral pathologists in reference to "lentor, alkalescency, fermentation and concoction and in the pride of opinion asks, "what has humoralism ever done for medicine?" This question he answers himself. "It has retarded its march, it has shut up the avenues to its successful cultivation, and deteriorated the genuine spirit of correct investigation."

We ask, is it fair to treat a grave doctrine by one fell swoop of denunciation? We apprehend not. If the author had cited the facts upon which the modern humoral pathologist rests the truth of his doctrine and refuted them by sound argument and correct logic, it would have been well; but he has repudiated them as the "chimeras dire" of a fruitful imagination. On page 47, he says:

"A boundless terra incognita, as I have stated in another place,* is presented by the mass of circulating vital fluid, contained in the arteries and veins of the animal machine, which our fond visionaries and romance writers on medicine have peopled with every variety of direful foes, to the weal of the body. Sometimes animalcula are made to sport in cruel triumph along the conduits of life; at other times, the blood is wrought into woful chemical changes, and is thickened into a dark spissitude or lentor, or thinned into a mere cruor, according as the mind of the medical novelist is troubled with 'thick coming fancies,' or is elated by etherial visions. One will gravely write by the hour, of impregnations, foreign infusions, and certain mysterious, but yet unappreciable

* Harrison on Cholera—Baltimore Medical and Surgical Journal; July, 1834.

changes in the vital fluid. Another will dwell with infinite complacency on the horrible blackness of the blood, which he finds dark as Cerbebus, and fit only for congestions, collapses, and death. But a still additional writer shall entertain his readers, very little at his own expense of research, by expatiating over the ground of an ill-defined and misty space, from which he imagines he descries in clear perspective, both territories of humoralism and solidism. Such a ready scribe speaks you fair, and tells you in grandiloquent phrase, of the possibilities, nay, absolute probabilities, of the blood becoming corrupted, and refers you for proof, not to the chemical analysis of any poisonous products found in the circulating mass, but to the experiments of some gentle vivisectors, who veritably force, by their torturing process, foreign materials into the blood vessels. Medicine is *called* by these gentlemen, a demonstrative science—aye, they emblazon it by the proud titles of an inductive and certain science,—a department of human knowledge, requiring at each step of its glorious march to ultimate perfection, a precision in the collocation of its facts, and a fixedness in its positions, that can never be impaired by the revolutions of time. Fond conceit! Is not instability written on the face of our science, and shall they who yet foster the prejudices of the past, in favor of a doctrine that never has been proved, idly dream of the ultimate achievements of truth over error, when they are engaged in rebuilding the crumbling fabric reared in the dark ages of human knowledge! We demand the proofs—we require, before we give our belief to the undigested doctrines of humoralism, to see something like a rigid and scientific appeal made to analysis and demonstration. Who ever appeals to the chemical changes of the elementary constituents of the blood, either during life, or after death, for evidences of the state of morbid action that may be present, or has left its ravages behind! It is evident that an incalculable amount of false experience in medicine, must have been reared upon the conceptions of disease, and the modus agendi of remedial agents, adopted by the defenders of the humoral doctrine;—such conceptions, in the language of Cullen, having contributed nothing to improve, and having often misled the practice of physic.

"Andral, who, to a very limited extent, is a humoralist, candidly avers, "that in the present state of the science, it is the part of a sensible man not to adopt the doctrine of humorism too lightly, by judging from facts, many of which require a re-examination before they are finally admitted, and that we ought to be particularly on our guard against being in too great a hurry to make practical applications of it.'* "

Here we conceive the author to be not so felicitous as usual in his reference to M. Andral; the support he expected his views of solidism to derive from this great anatomist and pathologist would have been swept from him had he even proceeded to finish the quotation begun, much less to refer to Andral's entire chapter on the lesions of the blood.

* Andral's Anatomy, vol. 1, p. 419.

It has been ascertained beyond contradiction that the blood does undergo extensive alterations; this has been established by actual experiment as well as by chemical analysis.

* M. Beclard cites a case in which he detected a solid clot of blood filling the heart and trunks of the large vessels, the interior of which presented a mass of encephaloid matter. Velpeau also mentions an instance of a mass of encephaloid concealed in a clot of blood filling the vena cava. In his *Anatomie generale*, Bichat reports a case in which the vena porta and the hepatic and splenic veins are filled even to their most minute extension with a greyish sanies instead of blood. The experiment has been instituted again and again of injecting the veins of one animal with the blood of another, both being in health and no injury resulting and yet when the blood from a diseased individual has been thrown into the veins of one in health consequences the most frightful have resulted.

M. Gendrin in his work on inflammations and in another on fevers reports many experiments made upon animals by injecting into their veins and cellular tissue, blood abstracted from patient's laboring under putrid fever and others afflicted with small pox. In these cases the most severe symptoms were speedily manifested and death soon ensued.

Now if experiments conducted in the true spirit of philosophic inquiry by different individuals at different times are to be relied on, do they not conclusively establish the position that the nature of the blood is altered, and if changed why will not a corresponding alteration be produced upon the solids? Can the solids exist without their regular and constant supply of blood? neither can the blood be formed without the intervention of the solids. Here then is the establishment of a beautiful reciprocity of action for the purpose of imparting life and animation to every tissue and organ such only as could have been effected by the great architect of nature.

"Thus then in the three-fold respect of the vital phenomenon, intimate structure, and chemical composition, "says Andral," no line of de-

* Andrel's Anatomy.

marcation can be drawn with strictness and precision between the blood and the solids. Physiologically speaking, it is impossible to conceive that one of these two parts of the same whole could be modified without the other being so likewise. On the one hand, inasmuch as the blood nourishes the solids, and as without its presence they cannot support life, the state of the solids cannot but be influenced by the state of the blood. The chemist might as well say that the nature of a body does not depend on the nature of the elements that compose it. On the other hand, the solids, considered with respect to their relations with the blood, form but two classes: the one contributing to *make* the blood, such as those concerned in the actions of absorption, digestion, arterial circulation, and respiration; the other contributing to *unmake* it, those, namely, concerned in the actions of venous circulation, secretion, and nutrition. No one solid, therefore, can undergo the slightest modification, without producing some derangement in the nature or quantity of the materials destined to form the blood, or to be separated from it. Physiology, then, leads us to the conclusion that every alteration of the solids must be succeeded by an alteration of the blood, just as every modification of the blood must be succeeded by a modification of the solids. Viewed in this light, there is no longer any meaning in the disputes between the solidists and the humorists; the system appears to constitute but one great whole, indivisible in the state of health as well as in that of disease; the division of the parts of the body into solids and fluids, seems to be a distinction of small importance, and one that is not always just, since it ceases to exist in the intimate structure of the organs, in which all the grand vital phenomena take place, and in which also, occur all the changes that constitute the morbid state.

"This intimate and necessary dependence of the blood and solids on each other having been once taught us by physiology, what remains to be done? Merely to have recourse to observation for facts, and to draw legitimate conclusions from them."

We have neither time nor liesure to pursue the investigation of this subject further at present neither will the space allotted to this review admit of its further extension.

Method of acquiring true experience.—It is here the author has shown himself master of his subject. The clear, beautiful, and interesting manner in which he has placed this subject before the mind of the learner will at once arrest his attention and exhibit to him the fact that it is only by patient industry and perseverance in the pursuit of medical knowledge that he can ever hope to arrive at distinction in his profession. The author happily remarks that

"To the formation of a sound discriminating judgment in practical medicine, there can be no substitute for clinical observation. Whatever attainments the student may make in anatomical, physiological, and pathological knowledge, still he must have his faculties exercised at the bedside, to enable him to acquire a just and useful experience in the art of curing disease. There can be no substitute for this most

essential mode of gaining a knowledge of the symptoms and nature of the various maladies to which the human economy is subject. All other acquisitions are only preparatory to the acquirement of clinical experience. Without witnessing the diversified phases of disease, as they reveal themselves substantially and truly at the snffering patient's couch, all our attainments, however valuable as a preparation, are useless as an end. As a terminating point of practical utility, it is very patent to the most obtuse interest, that the most accurate views of the organization of man's composite frame, or of the laws of healthy action established in that organization, or even of the laws of morbid action, could never impart enlightened views of the symptomatology and therapeutics of disease. No one can be a scientific physician unless he is intimately acquainted with the anatomy and physiology of the body—but neither can he be a judicious and safe practitioner, unless he superadd to all such most necessary preparatory knowledge, the knowledge of disease as it is derivable from seeing it—and of realizing its presence and power, and many changes, by an actual examination of the suffering fellow being, whose system is prostrate under its powerful agency. We cannot insist too strenuously on this point—it is of vital consequence to the growth of a legitimate experience in medicine—and without it, without clinical observation—whatever embellishments collateral branches of knowledge may pour around our art—whatever most necessary investigations may be had into anatomy, physiology, and pathology, as these fundamental divisions of our science are learned in dissections, lectures and books, yet at last the student of medicine must acquire for himself, by an exploration of the phenomena of disease at the bedside, the crowning excellence of all useful experience."

The entire book is constituted of four remaining essays and lectures each worthy of a separate and distinct notice; but for the sake of brevity we will speak of them as a whole and recommend them to the attention and perusal of students of medicine and at the same time express the hope that our elder brethren of the profession will read them and profit much by the instruction they contain. C. R. C.

Art. IX.—Diseases of the Bones.

The Anatomy, Physiology, and Diseases of the Bones. By Samuel D. Gross M. D., Professor in Cincinnati College. J. Grigg, Philadelphia, 1830.

Books written upon the several branches of medical science have been so vastly multiplied, within the last quarter

of a century, that one is sometimes ready to inquire, whether the state of our science still requires such continually accumulating additions to our libraries?

The medical reader who has kept pace with the advancement of the science, for the last half century, will readily admit that there has been, and continues to be ample room for pushing our inquiries vigorously, into every branch of medical science; but it is nevertheless usual and proper, that the medical inquirer should expect every author to acquaint us with the design, and particular end in view, in putting forth a new work.

In compliance with this usage Professor Gross has given such explanations as he deemed relevant to the publication before us. We are told in the preface that "excepting the lectures of Boyer, by M. Richerand and translated by Dr. Farrell, there is no treatise in our language in any degree commensurate with the subject before us. The works of Wilson, Brodie, Bell, Cooper, and others, although replete with information, are nothing but monographs, intended for the experienced practitioner rather than for the inexperienced student. In our own country, the diseases of the bones have hitherto received but little attention."

We believe that the foregoing assertion was not only correct at the time it was made, but that the condition of this matter has not been materially altered up to the present time —wherefore, we may readily acknowledge the utility of the work; and, we owe, to the author, more than our thanks for this successful effort to improve a very important subject.

"To supply deficiencies, and to furnish the student with a plain, concise and practical digest of the state of our science, embracing an account of the most important facts and observations, are the objects of the present undertaking."

We have been led, after a careful perusal, and mature reflection, on the work of Professor Gross, to aver, that he has well filled up the measure of his promise. The digest here presented is well calculated to introduce the inexperienced reader into sound elementary knowledge, in physiology, and pathology; nor will the experienced find a more ready book

of reference, for what they may have forgotten, or may have escaped their notice.

Our author very modestly tells us that he makes no higher claim than that of a faithful compiler. In this we willingly offer our testimony to bear him out. The work contains a very considerable mass of highly useful matter, judiciously put together. Such being the character of the work, it will not be expected that we should undertake an analysis of its contents. We deem it sufficient in this place to state, that it exhibits, in general, correct principles, and the best established rules of practice. Whenever the author has advanced new or peculiar opinions, they are sufficiently pointed out.

We purpose, in turning over the pages before us, taking brief notices of what we may deem remarkable; and, candidly stating our views of such points as we may consider of sufficient interest, whether from novelty, defect of knowledge as generally understood, or belief of improvement.

At page 10, our author alludes to the fact, that the nerves are not to be traced into the substance, or interior of the bones, and, in the succeeding page, he states, as his opinion that "the nerves of the ossious tissue are conveyed to the bones through the medium of the arteries, that they enter the same foramena, and that they are, as it were, concealed in their coats, so as to escape our observation: this opinion, I find, is also entertained by J. C. Rosemuller, formerly professor of anatomy, in the university of Leipzig."

There can scarcely be a doubt of the truth of the above opinion. We suppose every physiologist of the present day, will consider a nervous influence indispensable to the vitality of every organ, or tissue, of the animal body. It has been wisely contrived, that the parts which are subject to much mechanical attrition, either as supporters, or articular surfaces forming joints between the bones, as well as the tendons and ligaments, which are liable to violent extension, should be endowed with but little open or free sensibility; and hence it is, that nature has not supplied these tissues with nerves, in any degree proportioned to those distributed to parts, part of the natural function or office of which requires perceptibility in more or less force.

Now, it is well known, that some of the inferior animals; and the germinating rudiments of the more perfect animals, up to a certain period, exist and are developed with more or less rapidity, through a sensory having an interstitial placement co-extensive with the size, and having the form, of the entire animal body, whether in its various stages of growth, or after its completion. Then as the ulterior discovery, in the examination of animated beings has been to resolve them into molecules, we adopt the term molecular medulla as the name of a nervous matter, which is blended in the organization of every tissue, and in union with an influence pervading it at every point, exerted by the brain through the medium of the nerves.

We believe, that to this modification of the sensorial organs, associated with its share of the vivific principle, may be ascribed the fact of Haller's originating the opinion of a vis insita; and to it may we ascribe the plastic operations. I could not within the limits assigned this article, examine into this subject; we shall for the present content ourselves with a brief description of our views of the manner, in which the bones are associated with the more sensible parts or tissues.

We are not among those who ascribe the nervous energies to electrical influence, operating by any of its known laws, but we believe, that the nervous cords and fibres arise from the brain of the head and spine, and terminate in medullary substance, placed in every tissue; and, thus, in some degree, resembling the positive and negative electrical poles. The admission of such an arrangement will enable us to explain the manner by which the least sensible tissues hold their relative intercommunion of energy, life, health, or disease. Since pricking the skin with the point of the finest cambric needle, will cause severe pain, we may reasonably suppose, that no fibre is too small to give place to nervous influence—serving in the case before us, merely as conducting links, between the brain, as positive, and the medulla interstitially placed as negative points.

Now, we know that arteries are plentifully supplied with nerves, that these penetrate their coats, where the vessels and

nerves are of sufficient size, to come within the power of our vision; can we then, for one moment doubt the assertion, that nerves having started, so to speak, along the arteries, will continue to the termination of the arteries, however minute they may become—indeed, to doubt under such circumstances the fact here advanced, can be nothing less than absurdity.

But it does not seem necessary that we insist upon the distribution of nervous fibres, through the more insensible structures—it is sufficient for our belief, of a capacity in these organs for assuming open sensibility, to know, that nervous fibres do at least reach the surface of such organs or tissues; and, may therefore, convey nervous influence, by means of these fibres, as conductors.

For ourselves, we have no doubt of the opinion, that nerves having set out to travel, so to speak, with the arteries, never end until they have reached the termination of the arteries, in all parts of the same individual; but, we think it highly probable, that a nervous influence exists between the uterus and the fœtus, by means of nervous points terminating on the inner surface of the uterus, and operating upon the nervous system of the fœtus, by an intervening molecular medulla existing in the placenta, and unbilical cord. There is strong hope, we think, that the views which we have presented above, will be ascertained to be correct, by experiment.—Professor Schlemm, of Berlin, has demonstrated the existence of nerves branching into, and through the cornea. The particulars of this discovery may be seen in Jameson's Medical Recorder, vol. 2, page 148.

Professor Gross, at page 16, speaks of the well known condition of the insensible tissues, which renders them liable not only to be thrown into the state of perceptibility, but of supersensation. Who, that has witnessed this state of things can doubt the existence of a sensorial material in those tissues. But as we have already said, nature has wisely suited the inherent sensory of these structures, to their exposed condition; instead of degrading them to a state almost inanimate, as has too often been imagined.

It would seem more consistent, with our present views of phys-

iology, to distinguish sensibility into different modifications; and, we shall find, that on the one hand, nature designed that perceptibility should exist, as a condition through which organs or tissues were to perform their offices; so, on the other hand, where it was evident that perceptibility would go to unfit parts exposed to strong mechanical forces for their offices, they were made to possess a diminished amount of it.

Parts subjected to volition are endowed with a high state of sensibility, this may be denominated free or open sensibility. Parts not subject to volition have impressed upon them a capacity to take on sensibility when invaded by disease, this may be termed sub-sensibility.

Page 36 it is said—"Dr. Physick the distinguished Professor of Anatomy in the University of Pennsylvania, is of opinion that the periosteum often prevents the bones from participating in the diseases of the neighboring parts, in the same manner "as the pleura turns off an abscess in the parietes of the thorax from its cavity, or the peritoneum from the cavity of the abdomen."—(*Wistar's Anatomy.*)

We are aware of the truth of the fact here pointed out, —but it by no means applies specially to the periosteum, and pleura, it was well known to Mr. John Hunter, as a general law; and served to form one of his distinctions of sympathy. Simple abscesses do not materially involve the blood vessel, over, or near, which they are situated; inflammation, of the scrotum from injections into the cyst of the hydrocele seldom extends to the testicle; inflammation of the anus never extends from the skin to the rectum—suppurations in the perineum will expose the urethra for two or three inches, and leave it almost free from the effects of the suppuration, etc., etc.

This compendium contains *two parts—Part* 1*st, is arranged in three chapters.* The first chapter treats on the anatomy and physiology of the bones, and their membranes.

In this chapter, of upwards of thirty pages, we have presented a judicious selection of what is known on the subjects therein treated; and, we hazard nothing in saying, that no work can be found in which a greater amount of correct in-

formation can be seen within the same limits—the arrangement and matter are good, and presented in language well suited, being alike brief and lucid.

The second chapter treats on fractures or injuries of the bones. Section 1st of fractures in general. 2d—of particular fractures.

Chapter third, treats upon diseases of the bones. Section 1st, of caries—2d, of necrosis—3d of osteo sarcoma—4th, of spina ventosa—5th, of osteo hydatical tumors—6th, of rachitis—7th, of hydro-rachitis—8th, of mollities ossium—9th, of fragilitas ossium—10th, of periostosis—11th, of exostosis—12th, osteo-sanguineous tumors—13th, insterstitial absorption of the cervix femoris.

The three foregoing chapters contain a considerable collection of information highly interesting, since it embraces views of the injuries, and diseases treated of, as well as modes of treatment, embracing whatever is well established, by the most experienced writers, on the several subjects embraced within these chapters.

Part second is also arranged in three chapters. Chapter 1st, treats on the general anatomy of the joints. Sections 1st, 2d, 3d, and 4th, treat on the anatomy of the cartilages membranes, articular fluids, and ligaments.

Chapter 2d, of injuries of the joints. Section 1st, of dislocations in general—2d, of particular dislocations—3d, of wounds of the joints—4th of sprains.

Chapter 3d, of diseases of the joints. Section 1st, of inflammation of the synovial membranes—2d, hydrarthrus—3d, of inter-articular cartilaginous concretions—4th, of fungus articuli—5th, of fermero-coxalgia—6th, loxarthrus—7th, of anchylosis, etc.

The mere inspection of the above catalogue of injuries, and other diseased states of the bones, membranes, joints, etc., is sufficient to satisfy every one, that if these various affections and injuries, are properly managed, in regard to their characters and modes of treatment, it follows, as a matter of course, that the work before us contains a very considerable amount of information. I know of nothing of

equal extent, that can be obtained from any quarter whatsoever, containing more practical matter than the compendium here offered of the physiology, and pathology of the bones etc., that it contains a judicious selection of what is most valuable, I most willingly state as my opinion. I shall be pleased to learn that merit may find a reasonable and just reward in the free discrimination of this truly useful work. H. G. J.

Art. IX.—Self-Limited Diseases.

A Discourse on Self-Limited Diseases, delivered before the Massachusetts Medical Society, at their Annual Meeting, May 27, 1835. By Jacob Bigelow, M. D. Professor in Harvard University. pp. 48.

The object of this neat little discourse, is to make a recognition, and attempt a classification, of those diseases which, in despite of any method of treatment hitherto discovered, are known to run through certain *stadia*, when they either prove fatal, or cease, and the patient is restored to health—though sometimes more or less scarred, or permanently altered in the structure of the affected parts. The existence of such maladies has long been perceived, and they have, generally, been held to be incurable. We have, however, heard a physician, of the Broussais school, affirm that by the antiphlogistic treatment he had arrested the small pox, and caused it to abort—that is, prevented the eruption and the secondary fever. That the extent and intensity of these stages may be much reduced, has been known ever since the cold treatment was introduced; but we entertain serious doubts of the possibility of terminating the disease, after the contagion has taken effect. Should this and the other "self-limited" diseases, as Prof. Bigelow styles them, ever be cut short, it will, we predict, be done by other agents than those which are purely antiphlogistic—by means which establish in the system such an altered state of

the vital properties, as is incompatible with the continuance or the spread of the morbid actions previously set up. To the efficacy of such agents the predisposing influence of depletion may be important—sometimes indispensable, as we observe that the cinchona will not cure intermittent fever, nor the mercurial preparations, syphilis, at least with any degree of certainty, or comfort to the patient, if there be plethora and a high tone of inflammatory action; while they never fail after due depletion.

"The class of diseases under consideration, comprehends morbid affections, differing greatly from each other, in the time, place and nature of their spontaneous developments; so that they may admit of at least three general subdivisions. These may be called, 1st. The *simple;* in which the disease observes a continuous time, and mostly a definite seat: 2d. The *paroxysmal;* in which the disease, having apparently disappeared, returns at its own periods: and, 3d. The *metastatic;* in which the disease undergoes metastasis or spontaneous change of place. In the present state of our knowledge, we have no difficulty in finding examples of each of these subdivisions. There are also other examples, in which the disease, although capable of being in part influenced by medical treatment, still retains a portion of its original intractability, and has strong relations to the class in question." p. 11.

Under the first head, Dr. B. arranges *hooping-cough*, *measles*, *scurlatina*, *small pox*, *erysiplas*, and doubtingly, *typhus*. From his observations on *scarlatina* we extract with approbation the following sentence:—

"The modes of treatment, which have had most testimony in their favor, are various, and opposite. By Dr. Fothergill, stimulants were relied on; by Dr. Currie, cold water; by Dr. Southwood Smith, and others, blood-letting. But it is not satisfactorily shown, that either of these modes of practice has been particularly successful; for where the writers have furnished us any thing like definite, or numerical results it does not appear that the mortality was less in their hands than it is among those who pursue a more expectant practice. The post mortuary appearances, which in many diseases furnish useful lessons for practice, are in scarlet fever extremely various and uncertain; and sometimes no morbid changes, sufficient to account for death, can be discovered in any of the vital organs, or great cavities." p. 14.

Erysipelas is not generally regarded as a self-limited disease, but every physician must have met with cases in which

the inflammation could not be arrested till it reached the extremity of the limb or member on which it appeared. Thus, commencing on each temple, we have seen it deliberately advance forward, resisting all constitutional and local treatment, till it passed off at the tip of the nose.

The self-limited character of typhus is more doubtful. The period of its duration is exceedingly various; but this may perhaps arise from the varying intestity of action of the remote cause. Our author very strongly inclines to the conclusion, that typhus c n ot be shortened, and even doubts whether it can be broken up in its forming stage. For ourselves, we are compelled to confess, that we have seldom seen this disease more than mitigated, by any plan of treatment, to which it has been subjected.

"We come next to a second order of self-limited diseases, of which the term *paroxysmal* is sufficiently descriptive. This term applies to certain morbid affections, which recur in fits or paroxysms, leaving the patient comparatively well in the intervals, at the same time that the paroxysms themselves can neither be foreseen, prevented, nor, as far as we know, materially abridged in their duration." p. 21.

Under this division is classed *epilipsy*, *angina pectoris*, formerly *gout*, *mania*, *melancholy*, *asthma*, *gravel*, and *ascarides* in the rectum.

"Cases, which bear the names of all the above diseases, are undoubtedly relieved, and sometimes even removed by medicine; but it is equally true, that other cases are wholly intractable, both as to their recurrence, their duration, and their susceptibility of much change from medical treatment. And it will come to the recollection of many practitioners, that they have, in the course of their lives, believed themselves to have cured these diseases, when in fact they have only witnessed the spontaneous subsidence of a paroxysm." p. 23.

Our author's third general head embraces *metastatic* affections, of which he enumerates several.

"By this term I wish to express certain morbid affections, which pass by metastasis from one part of the body to another, for the most part independently of artificial influence. Of this kind are certain *cutaneous* affections, more especially some which are chronic and hereditary. Many persons pass a considerable portion of their lives in alter-

nate annoyance from a disease of the skin, and from its vicarious substitute in some internal organ. Others again are afflicted with hemorrhagic, or purulent *discharges*, which at times disappear, only to be succeeded by equally troublesome affections in a different part. *Gonorrhœa* cannot be prevented from occasional metastasis of inflammation to the testes, and *mumps* are sometimes found to undergo the same transition. But perhaps the most remarkable example of a metastatic disease is found in *acute rheumatism*. This morbid affection often begins to discover itself in a limited and comparatively unimportant part of the system. From thence, in grave cases, it travels by successive migrations from joint to j int, and from limb to limb, till it has visited nearly all the great articulations of the body. It also attacks the organs of sense, and the viscera which are essential to life. During the course of these migrations, the attending physician cannot foretell at any given stage, what part will be next invaded by the disease, neither can he protect any part from being thus invaded; nor can he control the period, during which the disease will reside in any particular part previously to its next metastasis. And in alarming and dangerous cases of this disease, it often happens that the physician can do little more than stand by with his palliatives, in the anxious hope, that the disease will at length pass by another metastasis, to some less vital, or less important organ." p. 24.

Dr. Bigelow has added to his discourse a variety of instructive notes, one of which is on verminous diseases, with an extract from which we shall close this notice.

"It is commonly believed that the principal residence of ascarides is in the rectum, because they are most felt there. They have been found however, in every part of the alimentary tube. Many patients, immediately after a cessation of the annoyance in the rectum, are visited by pain in the epigastrium, attended with costiveness and clay-colored discharges. This state continues for two or three days, and is then followed by a bilious diarrhea. I have repeatedly known these consecutive events to occur with great regularity for half a dozen years, so much so that my inquiries are generally directed towards this cause, when children have complained of epigastric pains at regular periods. Whether, in these cases, the worms ascend to the duodenum and mouth of the biliary duct, or whether the whole is an affair of sympathy, future autopsis may perhaps determine." p. 44.

MISCELLANEOUS INTELLIGENCE.

ANALECTIC, ANALYTICAL, AND ORIGINAL.

I.—ANALECTIC.

Ad Sylvas Nuncius.

1. *On the Employment of Sheet-Lead as a Dressing to Promote the Cicatrization of Wounds and Ulcers.* By Reveille-Parise.—The method of dressing with sheet-lead, appears to me much better adapted to effect the object proposed, than the ordinary means. The plan adopted is, to cover the wound, already in a condition to heal, with a piece of sheet-lead, of an extent and thickness to be determined by the size of the diseased surface, and other circumstances to be mentioned presently. It should be properly confined by means of a compress and bandage, or by strips of adhesive plaster, when the bandage alone is not sufficient. There is no species of dressings more simple, expeditious, comfortable and convenient than this. The softness and pliability of the lead, allow it to adapt itself readily to the shape of the parts, while the same properties render it quite as easy to cut it of the proper shape and dimensions, as an ordinary piece of lint, and at the same time make its application exceedingly easy. That the metal used in this way is innocuous, we think will not admit a doubt, since the deutoxide of lead, or litharge, has been long employed for the purpose of cicatrizing wounds and ulcers. It must not be inferred, however, that the sheet lead, like these substances, acts in virtue of any specific properties; its operation is purely mechanical. That this is the case, is evident from the fact, that I have obtained the same effects from the employment of plates of tin, silver and gold. The ductility of the lead, however, and its cheapness give it the preference.

An enumeration of all the advantages of this method of dressing, will suffice to show that it cannot be necessary to resort to unguents.

What, in effect, can such applications accomplish? The object is not to calm irritation and promote suppuration, but to obtain cicatrization. As the adherence of the borders of the wound is not to be dreaded, applications of unguents and cerates are altogether useless. The non-adherence of the dressings to the edges, and to the diseased surface, is an important advantage; because there is no pain experienced in the removal of the dressings; the newly formed cicatrix is not liable to be torn up, and the wound is kept clean and free from crusts and particles of ointment and charpie, which are apt to adhere to it when other dressings are employed.

I shall not swell these observations by a detail of individual cases; a few examples, however, may be cited in order to illustrate the general precepts. M. de S. in descending a stairs, received a slight wound in the heel, which refused to cicatrize. The application of ointments and plasters increased the irritation and swelling. Fine charpie, at first dry, afterwards endued with cerate, adhered to the surface, and even produced greater aggravation than the other dressings. At this juncture I was called. A piece of sheet lead was confined upon the part by adhesive strips, and the ulcer cicatrized in a few days.

These dressings can be applied with exceeding ease and promptitude, and they need not be so often repeated as those in common use; since they are not attended with that heat and humidity of the wound so favorable to decomposition, nor with itching irritation so apt to be excited by unctuous substances. It is easy, therefore, to conceive how by this procedure the healing process will be promoted, there being nothing to interrupt its progress. The operations of nature, when not thwarted in their tendency, and when aided by the co-operation of art, set aside the obstacles to the healing process, and advance steadily in the accomplishment of the cicatrization of ulcerated surfaces. These powers are never more clearly displayed than when the lead dressings are employed.

To these advantages may be added another: in the employment of the plates of lead, those formidable infections are never to be dreaded, which sometimes take place in using old and inferior charpie. Economy is likewise a consideration of some weight, the more especially, as the price of the best charpie is daily increasing, and even at present, it is a source of such heavy expense to large establishments, that attempts have been made to diminish this heavy expenditure. The means which I indicate, is as simple as it is effectual. A single plate of lead, if not too thin, will not only serve for the entire cicatrization of one ulcer, but may be afterwards employed on others,—the only precaution necessary being to cleanse it carefully, and slightly polish its surface.

These are the advantages of the mode of dressing ulcers which I propose to substitue for that in common use. They have been exemplified by the numerous applications I have made of the procedure in question, as well as by the experience of others, who since the publication of my first memoir have adopted it. So that theory and experience, in this particular, are in full accordance with each other.

At the time of my first publication, some surgeons, supposing that the leaden plates were only proposed for the cure of ulcers, did not resort to them for other purposes, and consequently obtained but partial or no success. Such was not my intention. My object was to show, that wounds and ulcers in the way to cicatrize; viz: those which are wide, superficial, red, in a state of free granulation, covered with a fibrino-

purulent secretion, and surrounded with a deep rose-colored areola, should be dressed and cicatrized by the application of plates of lead. It is only necessary to remove the dressings every two or three days, according to the abundance of the secretions.

On the other hand, when the wound is deep and presents no disposition to heal—when there is pain, profuse suppuration, especially if kept up by any peculiar virus, and when it is judged necessary to resort to local applications, as cataplasms, I question whether the method in question will prove successful: such at least is the result of my own experience. Nevertheless, it should be employed as soon as these obstacles are removed, and the part manifests a tendency to cicatrize. Notwithstanding the species of extension presented by many wounds, there is reason to believe that this plan of dressing may be employed in a majority of wounds. I may even subjoin, that there are cases, in which it will succeed in a manner somewhat different from that expressed above. Thus in many abscesses and encysted tumors, after the contents have been evacuated by a small puncture and the application of a cup, if a tolerably thick plate of lead be applied to the surface, and confined by a proper bandage, the adhesion between the walls of the cavity may be obtained in a short time.

In all cases in which the diseased surface has been reduced to the condition of a simple wound, and the obstacles to cicatrization have been removed, the method proposed is superior to all others, and success is certain. It is nevertheless more particularly applicable to such as are superficial and present a great extent of surface, as is exemplified in burns. If they are slight the plates may be resorted to immediately; but when they are profound, time should be allowed for the accompanying accidents, and the profuse suppuration to pass away. These rules attended to, they all heal promptly under the application of a simple plate of sheet-lead, to be changed according to circumstances. The same may be remarked of certain blistered surfaces, in individuals of an irritable habit, and laboring under protracted diseases. Many practitioners have remarked, that such persons support with difficulty the application of charpie, adhesive strips, and plasters spread with cerate, to the raw surface. Each dressing is attended with acute pain, the surface bleeds more or less freely, and in some cases, fever and wakefulness are the result of these painful dressings, which always retard the process of cicatrization. All these inconveniencies may be effectually obviated by the simple application of a piece of sheet-lead.

This method of dressing is equally successful upon parts in which cicatrization is with difficulty accomplished; as the elbow, the crest of the tibia, the malleolus and the tendo achilles. Few surgeons have escaped the embarrassment experienced in healing ulcers of small extent upon these parts; by the new method they may be cicatrized promptly.

Dartrous ulcers, and indeed all dartrous and erysipelatous affections in a state of suppuration, cannot be healed by this method; but are ameliorated by it, when it is not found necessary to employ tonic applications, or when continuous medicated lotions are simultaneously used. Experience has demonstrated, that there are wounds with loss of muscular substance, which cannot be cicatrized by any means. The plates of lead, under such circumstances, have not appeared to me to be more successful; yet they can be applied with more convenience and promptitude than dressings of charpie. These advantages are greater than might be supposed in such wounds, which are thus covered with a kind

of protection, capable of serving as a substitute for the natural integuments.

It is universally known, that extensive cicatrices become lacerated with extreme facility, and that they afterwards heal with difficulty, either on account of the feeble vitality of the part, or some other cause. Hence the disadvantages of dressings of charpie and cerate; each successive removal of them tending to destroy the newly formed cicatrix, and in this manner, to perpetuate the sore. These inconveniencies are removed by the lead, though the cure cannot be so soon accomplished as in other cases. Even when the new cicatrix has formed, it may be strengthened by the lead; and it rarely becomes torn a second time under this species of protection.

The advantages of this new mode of dressing are fully signalized in wounds, ulcers, and erosions which take place in limbs affected with œdema or other species of engorgement. Experience has fully shown the insufficiency and even danger of charpie and cerate dressings in such cases, and their inevitable tendency to aggravate the disease. It may be replied, perhaps, that if seconded by compression, they will prove successful; but to this it may be objected: 1st. That compression does not diminish the inconveniencies of the application of charpie and unguents to the wound. 2d. That this compression is not always practicable. 3d. That the lead dressings, instead of opposing any obstacle to the employment of this means, favors, on the contrary, the attainment of its good effects. My experience has abundantly proved the truth of this assertion. The following case affords conclusive evidence. The Baron L——, of an active temperament, but broken down by age and the labor of the cabinet, was attacked with a chronic enlargement of the liver, which ended in dropsy, and finally destroyed his life. During the course of this protracted disease, his legs became tumid, even before symptoms of serous infiltration were apparent; the skin became red, sensitive, preternaturally thin, and was affected with frequent excoriations, which terminated in ulcers difficult to heal. These effects were produced by the friction occasioned by fine thin stockings, and his legs were covered with ulcers, which added to the pain of his situation. The finest charpie, the freshest cerate, and the most soothing dressings increased the mischief. Compression was also employed without success, and was so painful that it could not be supported. I placed the legs in a horizontal position, covered the ulcers with a thin plate of lead, and confined it with a moderately tight bandage: the diseased surface was soon healed, and as other ulcers formed they were treated in the same manner.

In some young children, blistered surfaces are very prone to ulcerate profoundly, and to increase in extent, in spite of all local applications. Fresh butter, cerate, goulard lotions, etc. often prove of no avail; but a simple plate of lead will often heal them in a short time. I was called to visit a child, to whose arm a blister had been applied for the relief of an attack of bronchitis. The blistered surface was so extensive, notwithstanding all the means that had been employed, that nearly the whole arm was excoriated. The child was also affected with acute pain, want of sleep, and extreme irritation. I covered the whole surface with a thin leaf of lead, which was removed next day, and a cure was speedily effected. It also often happens, that issues, and cauteries, after being established, cannot be sustained, on account of the preternatural irritation of the dermoid tissue, or some other inappreciable

cause. The contour of the part is surrounded by an erysypelatous blush, which is augmented under the use of ivy leaves, cautery paper, and other means usually employed in such cases. The application of a thin plate of lead for a time more or less considerable, remedies all these difficulties. In confirmation of this assertion, I could cite numerous examples, did my limits permit.

As a dressing to ulcers, properly so called, the plates of lead can scarcely be employed, except on the score of economy, and not for the purpose of promoting cicatrization, as long as the ulcer is perpetuated by some special cause, either local or general, which must be first removed. Varicose ulcers are, perhaps, those in which this remedy promises most advantages, especially when aided by pressure. Sheet-lead was employed successfully in these cases, by M. Marjolin, (Dict. de Med. Art. *Ulcere.*) By some practitioners, the method of Baynton, or the application of adhesive strips, is deemed preferable to the sheet-lead. I would reply: 1st. That this last method and the leaden plates are not employed under the same circumstances, and for the reasons enumerated above. 2d. That there are cases in which Banyton's method is impracticable; as, for example, when a leg is much swollen and infiltrated, and at the same time ulcerated at several points,—also, when the ulcer is seated upon any other part than the extremities.

The following are the rules I observe in the application of this method of dressing. I will suppose that one of the extremities has received a severe wound. After having examined its depth and extent, with the object of ascertaining that there is neither fracture nor dislocation, and removed all extraneous matter, should any exist, I attend to the disgorgement of the wound, either by suffering it to bleed freely for some time, by means of incisions, and especially by the repeated application of a cup with a piston attached. This latter procedure greatly favors union by the first intention, where it is practicable, and facilitates the healing of other wounds. The parts having been thoroughly disgorged, the subsequent inflammatory congestion should be combatted by the application of compresses dipped in cold water, or goulards lotion, either with or without the addition of laudanum, according to the intensity of the pain, and the susceptibility of the subject. When inflammation is fully developed, it only remains to moderate the organic excitation, and to counteract the local determination. All practitioners resort very properly, under such circumstances, to emollients in form of cataplasms, fomentations, embrocations, etc. which are continued until suppuration is established. This process was formerly divided into two periods: 1. *Preparative* suppuration, when the pus is serous and crude, to use the language of the times. 2. *Consolidating* suppuration. It would be better, perhaps, if this distinction were not neglected at the present day, since it is just, and is founded in fact. During the continuance of the first, I employ cataplasms, digestive pledgets, cerate, and other analogous means; but so soon as the second period arrives, I abandon the use of charpie, and all unctuous applications, thinking with Ledran, that it is not by medicaments introduced into a wound that it is cured, and that strictly speaking, all applications of charpie, or medicaments, should be regarded as a foreign body. It is under these circumstances that I resort to the sheet-lead, according to the regulations above described, which I continue until the wound is completely cicatrized, and some time afterwards, with the view of furnishing a salutary degree of support. This dressing should

only be removed every second, third, or fourth day, according to the abundance of the suppuration. When it is necessary to use pressure, the piece of lead may be a little thicker than ordinary, taking care not to have it so heavy as to occasion fatigue by its weight. Should the granulations become exuberant or spongy, and the surface of the sore remain stationary, the ordinary means must be employed.

I could cite the testimony of many surgeons in favor of this method of dressing ulcerated surfaces, but shall content myself with that of M. Yvan, surgeon in chief of the *Hotel des Invalides*. He not only employed the sheet-lead dressings with success in the cases indicated by me, but likewise in the treatment of hospital gangrene.—*Bulletin generale de Therapeutique.—Arch. of Med. and Surg. Sci., June*, 1835.

2. *Experimental Trials on the Efficacy of repeated Purgatives in Typhous Fever.*—Hamilton, of Edinburgh, was, we believe, the first physician who insisted on the efficacy of purgative medicines as a principal means of cure in typhus fever. His doctrine has been adopted by many practitioners; but by others, and particularly by the French, it has been combatted as a method which is incompatible with the pathology of that affection. In order to afford means for deciding this question, important for the value of the truth which affirmative decision may establish, and interesting from the degree of attention which the subject has so long excited. M. Piedagnel undertook a series of experiments at the *Hotel Dieu*, which were calculated to develop an answer to it, and he certainly has conducted them in a manner which is well designed for determining the true value of the purgative method. All the typhoid fever cases which he treated, were, without any distinction of symptoms, period of disease, etc., submitted to the action of purgatives during *the same season*. The number of patients experimented on was large, and no other active remedies, such as bleeding and leeches, were applied, these being avoided in order that the result might be as positive as possible. We now present our readers with an analysis of the paper on this subject, which M. Piedagnel has published in the 13th No. of the French *Gazette Medicale*, the paper having been read at the Academy of Medicine on the 24th of March.

The author distinguishes four kinds of typhus fever, viz:

1st. Simple typhus.

2d. Adynamic typhoid fever; to the common symptoms are superadded those arising from a considerable alteration of the intestinal canal, consisting in numerous ulcerations of the mucous membranes: here the abdominal affection becomes prominent, and the patient at the end falls into a state of adynamia.

3d. Ataxic typhus: here the cerebral symptoms predominate; there is a peculiar delerium, pain in the head; the senses are more or less perverted, and the muscles contract, etc.

4th. Putrid typhus (fievre typhoide foudroyante;) this form can only be likened to the effects of poisoning; in three or four days the patient dies, and the autopsy does not reveal any organic alteration.

Such are the very different forms of typhus fever which M. Piedagnel has submitted to the following treatment.

On the day after the patient's reception into the *Hotel Dieu*, the purgatives were immediately administered, when the symptoms were severe; if not, he was allowed to remain quiet for one or two days. When the treatment was commenced, a purgative was given every day,

or every second day, according to circumstances. The patient took for drink, water sweetened with syrup of currants, and his diet consisted of three *bouillons* (weak broth) per day. This regimen was invariably pursued; and the modifications of the treatment were very simple. Thus when a patient went naturally to stool, a slight purgative only was administered. In cases where a gentle purgative produced no effect, a stronger one was immediately given. The rumbling sound of the bowels, and particularly the appearance of meteorismus, were an indication for the employment of purgatives still more energetic. These means usually produced six to ten stools in the twenty-four hours. In some cases the patients were purged only once or twice during the whole course of the disease; in others ten or twelve times; but in general three or four purgative doses were sufficient. The state of the abdomen never furnished any contra-indication; thus a severe pain in some one point of the abdomen generally yielded to the first or second purgative, and never resisted the third. The purgative medicines employed were Eau de Seidlitz, from two glasses to one or two bottles; a solution of one or two ounces of Epsom salts; castor oil; calomel; and croton oil.

On comparing the results obtained by M. Piedagnel with those obtained in the different hospitals of Paris, they certainly present a very favorable aspect.

From the 1st of June, 1834, to the 1st of March, 1835, no less than 134 cases of typhus fever were treated by the author at the *Hotel Dieu*, all exhibiting in a greater or less degree the peculiar expression of the face, state of the mouth, rales moquex and sibilant, diarrhœa, pain of abdomen, petechiæ and sudamina, which distinguish that affection.

The cases may be arranged under the following categories:

1st. *Simple Typhus.*—69 cases. No death. Mean duration of the disease 20½ days. Mean duration of treatment 13½ days. Average number of purgatives 3½.

2d. *Adynamic Typhus.*—49 cases. Cured, 39; dead, 10. Mean duration of the disease 17¼ days. Mean duration of treatment 10¼ days. Average number of purgatives 3.

3d. *Ataxic Typhus.*—16 Cured, 7; died, 9. Mean duration of the disease 29 days. Mean duration of treatment 19 days. Average number of purgatives 6½.

Hence in 134 cases, we find 115 cured, 19 dead, giving the proportion of mortality as 1 to 7, 1-19th of the cases treated. But amongst the 19 deaths, M. Piedagnel enumerates two which ought not strictly to be included; one, cured of the fever and on full diet for four days, was cut off by a double pneumonia; the other, also cured, contracted the small-pox, which terminated in death. If we abstract these two cases, the general mortality will be very nearly 1 to 8. For adynamic typhus the proportion is 1 to 4 9-10ths,—the author of the memoir says 1 to 3 9-10ths, but he is evidently mistaken. Finally, he enumerates amongst the ataxic cases, the only two examples of *fievre foudroyante* which presented themselves in the course of the year; hence in this severe form the cures and deaths may be accounted exactly equal.

Let us now compare these proportions with the result of the practice of MM. Chomol and Bouillaud at the *Hotel Dieu* and *La Charite*.

Hotel Dieu.

	Patients.		Dead.		
In 1830	27	—	8	—	1 to 3.375
1831	56	—	16	—	1 to 3.5
1832	23	—	5	—	1 to 4.6
1833	30	—	10	—	1 to 3

At La Charite.

	Patients.		Dead.		
In 1834	31	—	5	—	1 to 6 1-6

Thus in the practice of M. Chomel the mortality is as 1 to 3.4871794. In that of M. Bouillaud, or rather in the small number of cases reported by him, 1 to 6.2: and in that of M. Piedagnel as 1 to 7.0526315789 47368421. (The decimal runs to this great length before it begins to repeat.)

Hence the author concludes that so far as regards the mortality, the treatment of typhus fever by purgatives is superior to any other practised at the present day; but it is extremely fatiguing for the patient, and requires extreme care on the part of the physician. The most frequent complications with which it may be reproached, are inflammations, which sometimes determine death; but, on the other hand, we very rarely find extensive gangrene, abscess, meteorismus, etc. and the convalescence is probably less prolonged.— *Lancet.— Bost. Med. and Surg. Jour., June*, 1835.

3. *On the Cure of Ascites by Stimulating Injection.*— In 1832, a favorite blood-hound bitch, the property of a friend, became dropsical, and had ascites. She was tapped, and a strong stimulating injection thrown into the peritoneal sac immediately after the evacuation of the fluid from tapping, considerable inflammation supervened, but the antiphlogistic measures adopted were successful in overcoming it, and a perfect recovery took place, the bitch never having any return of the disease, and frequently afterwards had young. After this, I was led to reflect on the benefit that might be derived from a similar mode of treatment, where the disease occurred in the human subject, and more readily came to the determination of trying it on recollecting the example although on a smaller scale, of curing hydrocele by a stimulating injection. The great objection offered to attempting the cure of acites after this mode, has been the fear of inducing an extensive and fatal inflammation of the peritoneum, but it did not appear to me to be attended with the quantum of risk so generally apprehended as serous membranes, however liable to be excited into an inflammatory action in a healthy state, are by no means so easily as when in that peculiar state wherein the serum is so superabundant as to form dropsy. On the contrary, they require the application of such remedies as will decidedly create an increased action, and will bear stimulating applications without injury to a much greater extent than we have hitherto allowed ourselves to believe. Good, in his "Study of Medicine," says Paullini relates a singular mode of operation, "the patient not submitting to the use of the trocar, had the good fortune to be gored in the belly by a bull, the opening proved effectual and he recovered."

The only conclusion we can come to on perusing such a case, is that the bull's horn having entered the peritoneal sac, allowed the collected fluid to flow out, and merely created such an increased action through-

out the weakened peritoneum as restored it to healthy and vigorous action, whereas had the peritoneum been similarly wounded when in health, the probability is that the action would have been so much increased as to have produced violent inflammation, rapidly terminating in death. In consequence of the train of reasoning I had adopted, I determined on trying a stimulating injection for the cure of ascites in the first favorable case, and the result of which I have now to detail:

In 1823, a negro slave attached to Williamsfield estate, by trade a cooper, elderly but healthy, was suddenly seized with general dropsy, a variety of remedies were used, and the general dropsy disappeared, but fluid was evidently collecting in the belly, the disease rapidly increased in spite of all the usual remedies, and soon required that he should be tapped for the relief of the more urgent symptoms. I tapped in presence of my friend Dr. James Rankine, now in extensive practice in England, and after drawing off a very considerable quantity of fluid, a quart of cold water in which I had mixed a wine-glass full of house rum, was thrown into the peritoneal sac; the patient was then placed in a recumbent posture, and means taken to have the injection applied as generally as possible to the whole of the sac, the patient stating in a short time that he felt some degree of pain and uneasiness, as much of the injected fluid as possible was drawn off, but certainly not so much as was thrown in. I watched the patient most anxiously, and the morning after the operation he complained of considerable uneasiness and some pain on pressing the abdomen, there was no fever or any material affection of the pulse. I had recourse to fomentations, and before evening all uneasiness had vanished. Suppuration to a small extent took place in the wound made by the trocar—the patient was put under a course of tonics, and good diet with gentle exercise; his recovery was rapid, and he resumed his usual work on the estate, and continued to labor for six or seven years, and then died of a disease not connected with his former dropsical attack. I regret I had not an opportunity of examining the body after death. The remarks I would now make on this case are few, it being a solitary one prevents my drawing a conclusion that further trial may prove to be fallacious; yet I cannot help feeling sanguine as to the ultimate success of curing ascites by means of stimulating injections when tried under favorable circumstances as readily as hydrocele is now cured by similar means. It can only be tried with advantage or hope of success in those cases where there is no extensive derangement of any of the chylopoetic viscera; where it arises in dirt caters, with whom there is generally disease of the stomach, liver and spleen, or in drunkards from indurated liver and lost powers of digestion; it would probably prove of no avail; where ascites supervenes, or debility from loss of blood or long continued fever, and from other causes not connected with visceral disease we may anticipate success from stimulating injections thrown into the peritoneal sac after tapping. At all events the above case will show, that a stimulating injection may be used without inducing the dangerous symptoms, the dread of which has hitherto prevented its being tried. I am not aware that any British surgeon has before this case, tried the effects of a stimulating injection in similar cases, and I shall feel proud indeed, should the case now recorded, be the means of future benefit to dropsical patients.—*Jamaica Physical Journal.—U. S. Med. and Surg. Jour.*

20th March, 1835.

4. *Hypertrophy of the Muscular Coat of the Stomach.* By Dr. Otto. —The records of science, as yet, contain but few examples of this pathological phenomena. Neither Morgagni, Haller, nor Baillie allude to it. Meckel, in his Manual, states, in a general way, that in great eaters the muscular coat of the stomach is often thicker than is usual. M. Louis has indeed detailed two well-marked instances of this hyperthropy which he met with; in both, the pyloric orifice was affected with scirrhus and he was thereby led to regard the former change as the result of the latter. A woman, 49 years of age, of a spare habit of body, had long suffered from mental anxiety, which she vainly endeavored to subdue by immoderate indulgence in the pleasures of the table; —in course of time the appetite became quite voracious. Her chief malady was a difficulty of breathing, which returned periodically every evening, and was accompanied with the sensation as if a heavy ball was rising from the lower part of the belly to the region of the heart, and there stopped, and obstructed the respiration. These symptoms lasted generally for about six or eight minutes, and then gradually subsided. Each attack became more severe; and at length they sometimes induced delirium. A general emaciation supervened, although the bulimia was not diminished. All the remedies tried to relieve her failed, and she died in the course of a few months from marasmus.

Autopsy.—Thoracic organs healthy; omentum almost entirely wanting; liver, gall-bladder and spleen natural. The stomach was puckered, and evidently much thickened, especially towards the pyloric orifice, and along the great curvature; its blood-vessels were empty: the mucous membrane, coated with a viscid deposit, was thin, transparent, but not softened; its surface was elevated into bold irregular projections, by the subjacent bundles of muscular fibres, extending from the great cul-de-sac towards the pylorus; between these projections were hollows, or depressions; and thus this stomach presented an appearance not unlike to that of the inner surface of the heart. The thickness of some of the muscular fasciculi was at least half an inch. The rest of the alimentary tube was normal.—*Archives Generale, from Hufeland's Journal, Feb.* 1833.—*Amer. Jour. Med. Sci., May*, 1835.

5. *Existence of Charcoal in the Lungs.*—Dr. G. Pearson, in a memoir in the Philosophical Transactions, for 1813, on the coloring matter of the black bronchial glands, establishes the fact, that a dark carbonaceous matter accumulates to a considerable extent in the above mentioned glands, during the course of life, in the human subject, which he thinks is introduced into the lungs with the air in breathing. The black matter was obtained not from the red glands, but from such only as were dark-colored, by acting on them indifferently with caustic potash or nitric acid, so as to dissolve or destroy the organic matters. This black matter proved to be a tasteless and infusible powder, insoluble in muriatic acid, nitric acid, and perhaps all common acids except sulphuric; affording as large a proportion of carbonic acid, when burnt, as animal or vegetable charcoal which has been dried at the same temperature, and equally unaffected by heat in close vessels. In fact, although not completely freed from animal matter by Dr. Pearson, the black powder possessed all the properties of carbon or charcoal, as we have it in lampblack or soot.

Dr. Pearson believes that this sooty matter is deposited on the surface of the bronchial tubes and air-vessels from the air inspired; and,

from a comparison of the black lines and black net-like figures, many of them pentagonal, on the surface of the lungs, with the plates of the lymphatic vessels, by Cruikshank and others, which they exactly resemble, he infers "that the lymphatics of the lungs absorb a variety of very different substances, especially this coaly matter which they convey to the bronchial glands, and thus render them of a black or dark blue color. When a thin slice of the surface of the lungs was treated with nitric acid, these black lines remained, as black threads of charcoal, after the solution of the other parts.

Dr. Graham, of Glasgow, has had several opportunities of substantiating the existence of a carbonaceous matter, in a state of extraordinary accumulation, in black lungs, and the *Edinburgh Medical and Surgical Journal*, for October last, contains an account of his investigations. From these he draws the following conclusions:—

1. The black matter found in the lungs is not a secretion, but comes from without. The *pigmentum nigrum* of the ox I find to lose its color entirely, and to leave only a quantity of white flocks, when rubbed in a mortar with chlorine water. *Sepia*, which is a preparation of the dark colored liquor of the cuttlefish, was also bleached by chlorine. But the black matter of the lungs was not destroyed or bleached in the slightest degree by chlorine. It even survived, unimpaired, the destruction of the animal matter of the lung by putrefaction in air.

2. This foreign matter probably varies in composition in different lungs, but in the cases actually examined, seems to be little else than lamp-black or soot.

3. It is evident that the accumulation of charcoal in the lungs, which Dr. Pearson remarks to occur more or less in every subject, may proceed in peculiar circumstances to a great extent, without affecting immediately the general health of the patient. In Dr. Laurie's and Dr. Buchanan's cases, all the patients were robust and healthy, had no cough, and no expectoration of black matter.—*Amer. Jour Med. Sci. May*, 1835.

6. *Case illustrating the Anomalous Nervous Symptoms, occasionally induced by Tænia.* By John Scott, M. D.—In the spring of 1830, I was requested to visit James Simpson, a pastry-cook, residing in Rose street. He had been laboring under symptoms of disordered stomach for a year and a half, and for the last nine months of that period the disease had assumed a serious form, he having been constantly affected with vomiting about an hour and a half after taking food, whether it was solid or fluid, unless in the very smallest quantity. He was also, from his wife's account, liable to fits, the nature of which I could not ascertain from her description. He had been under a most respectable practitioner during the whole period, and used a variety of means without benefit.

The color was sallow and unhealthy; the skin clammy; pulse soft and natural; some emaciation, but not to a great extent, or in proportion to the excessive weakness of which he complained; bowels very irregular; usually constipated: constant feelings of pain and gnawing in the epigastrium; not much increased on pressure. Different medicines, chiefly of the laxative kind, were used, when a smart attack of bowel complaint came on, during which a considerable quantity of blood, mixed with feculent matter, was passed, and then the vomiting ceased; the uneasy feelings disappeared, and he considered himself as well.

In about two months afterwards I was requested to see him again, when I found that the vomiting after food had returned, and that the bowels had become very constipated. He complained of pain over the whole head, which was particularly intense on the fore-part, increased by light and motion; the pupils were contracted; the pulse ninety-two, full and hard. There were intervals of ease, and he had occasionally a quiet but short sleep. Laxative medicines were given, and blood taken freely from the arm, with instantaneous relief, although of short duration, for the next morning the pain was as severe as ever.

A friend saw him with me, and we were somewhat puzzled whether to consider it as inflammation of the brain or not; but considered it prudent to repeat the bleeding. The appetite was gone, great thirst, but the liquid taken was immediately vomited. His wife said the fits had returned. It is unnecessary to enter into long details. In a few days the pain in the head was somewhat mitigated, though he still complained much of it. It was in visiting him early in the morning that I first witnessed what were called his fits. He lay in his bed with his eyes open and directed upwards. When I inquired how he felt himself, he did not look at me, but replied, "Quite well,—there is nothing the matter with me." "How is your head!" "Quite well." "No pain?" "No." "Any sickness?" "No." The pulse was calm and about ten beats lower than usual. When conversing with him he suddenly rose up in the bed, gave a long yawn, stretched out his arms, gazed at me for a moment, and then put his hand to his head, and complained of the pain in it.

A few days afterwards I found him dressed by the fireside, and scolding his wife for some alleged fault. When I inquired what the matter was, he said he had been very ill used, and that he had dressed himself to go and complain to Dr. Scott, and entered into long details, addressed to me as a third person, perfectly unconscious of my presence. The eyes were not directed to me but straight forward; and he always said he was quite well. On starting from this state, which was almost instantaneous, he complained of his head; the vomiting still continued; and he had a constant gnawing feeling in the epigastrium. I tried a variety of means, and witnessed several of the attacks, and there was one remarkable circumstance connected with them, that when awake he could scarcely be persuaded to take food of any kind, while in his unconscious state he complained bitterly that his wife had been starving him, and would give him nothing to eat, and when food was offered he took it eagerly. His natural sleep was rather sound, and he awoke in the usual manner; but generally in the morning relapsed into fits of unconsciousness, which continued from ten minutes to a quarter of an hour, and sometimes half an hour, and recurred three or four times a day. When questioned as to his sensations, he professed his utter unconsciousness of having been spoken to, or of my presence. Previous to his waking he frequently rose from his bed, dressed himself, walked about, and was with great difficulty prevented from going into the streets, generally for the purpose of visiting me. It now occurred to me that these strange appearances might be connected with worms, I therefore gave him a full dose of turpentine, followed by castor oil, which brought away many pieces of *tænia solium*, with much mucus. The dose was repeated several times, the worm discharged, and all the symptoms disappeared. He has had frequent returns of sickness, and other indica-

tions of his former disease, but these have been invariably removed by turpentine or decoction of the pomegranate root, and followed by a discharge of pieces of worm. For the two last years he has been perfectly well.—*Edinburgh Med. and Surg. Jour.*—*Ibid.*

7. *On the Organization and Origin of Intestinal Worms.* By Wm. Stokes, M. D.—(Extracted from his lectures on the theory and practice of medicine, delivered at the Medical School, Park street, Dublin.)—There are few subjects which possess so much interest in a physiological and pathological point of view, as that of intestinal worms; and in order to have correct notions, it will be necessary to be acquainted with the investigations of modern science on this subject. Worms are found in most classes of animals. They occur in reptiles, fishes, birds, in the different classes of quadrupeds, and in man. In man they do not exist in such abundance, nor so frequently as they do in birds and fishes. With respect to their places of habitation, we find them, first in cavities which have an external communication, and next, in the parenchymatous substance of organs; and we generally observe, that those which inhabit the cavities are different from those met with in parenchymatous parts. We observe, also, that the species existing in the different organs and cavities are not only different in their nature, but that there is a difference between the worms which inhabit separate portions of the same organ or cavity. In one part of a cavity or organ we find one species, in another a different, and this occurs almost invariably, as if it was regulated by a fixed law of the economy. A peculiar species of worm, occurring in man, called the *Distoma hepaticum*, is never found except in the liver or gall-bladder. If this animal had been introduced from without, it would certainly be detected in some parts of the intestinal canal, but this is never the case. Rudolphi states, that the *Strongylus horridus* is to be met only in the œsophagus of aquatic birds, and the *Ascaris obtusa* in the stomach of mice.

Generally speaking, worms are of three different forms—cylindrical, riband-shaped, and vesicular. Their organization varies from the lowest scale, in which we can scarcely trace, as it were, the rudiments of an animal; beginning with the tape-worm, which presents little more than a cellulo-gelatinous mass, we ascend gradually until we arrive at a high degree of organization, where we find well developed muscles, a difference of sex, generative organs, and, according to some anatomists, a tolerably perfect nervous system.

Now, to remove all sources of doubt and error on this interesting subject, and to establish proper principles of treatment, let us examine into the origin of these animals. I shall confine myself to the consideration of the origin of those worms which inhabit the human intestines, as they are the only species which we have to do with as practical physicians.

You will at once perceive that worms must be derived from one of two sources; either as introduced from without, or formed originally within the bodies of man and other animals. It is maintained by those who are in favor of the first supposition, namely, that they are introduced from without that similar animals are to be found in the external world, and that they are introduced either in the form of ova, or in a state of perfect development, with the food or drink, or by the respiration of the animal. Observe, this doctrine is founded on the validity of the assertion as to whether animals similar to intestinal worms are

to be met with in external nature. Linnæus states that he found the tape worm, and the small ascarides, a species now called *oxyuris vermicularis*, in a marsh in Lapland: but Muller, a much more accurate helminthologist, has since shown, most satisfactorily, that Linnæus was completely mistaken, and that those he had observed are never found to exist within any animal whatever. There are many observations on record similar to those of Linnæus: but as they were made at a time when natural history was in its infancy, and as they have been disproved by the researches of modern zoologists, I shall not notice them. I believe there is no well-authenticated instance on record of tape worms, lumbrici, or ascarides being found living in any situation external to the animal body. Every one of you, gentlemen, have seen worms in the intestinal canal, or recently discharged by stool or vomiting, but I will venture to say, that not one has ever observed them in any article of food, in earth, or in water. Bremser, who is a high authority, makes a very pertinent remark on this subject. "We find," says he, "all animals most abundant in that situation which has been assigned to them by nature. Now, if these animals were accidentally introduced from without, we ought to find them more abundant in the earth, water, etc.; but the contrary we have seen to be the fact."

But it is contended that these animals may have been introduced from without, and that in consequence of a change in situation, nutriment, and other circumstances, their forms may be altered; and it is argued in support of this hypothesis, that external circumstances will and have been observed to change the forms of plants and animals in a very remarkable degree. In addition to this it may be said that an alteration in the nature of its food may even produce an actual change in the function of the animal. It is a singular fact that neuter bees may be made prolific by changing their food; it is shown that when a queen bee dies or is lost, the neuter bees take a grub of their own species in place of her, and by feeding it in a particular manner, it becomes capable of laying eggs.

Now supposing that intestinal worms are introduced in the form of ova into the human body, there is no reason why this sudden, remarkable, and complete change should take place. We see nothing similar to it in nature. The plant which springs from any particular seed will resemble that from which it derives its origin; the egg of any particular bird, no matter in what way it may be hatched, will produce an organized being similar to its parent. The form and character of the animal are given during the act of generation, and remain unchanged.

Again, admitting that a difference in circumstances and nutrition might produce a total change in form, it should be in our power to demonstrate the individual in the process of transition; we should find those animals in a state half between what they were and what they are, and this state we should observe of very frequent occurrence. No such thing has been ever demonstrated. Out of a vast number, Bremser did not find a single one in any stage of transition, nor has it been demonstrated by any zoologist. He also states expressly, that after having diligently examined 15,000 specimens of worms in the cabinet at Vienna, he never was for one moment at a loss to say which were intestinal worms and which were not. If there was any such transition it would have been discovered, but no such thing has ever been observed.

It appears then obvious that there is no direct evidence to prove that these animals have been introduced into the body from without, either in the form of ova or in a state of perfect development. We have nothing then, I think, but to come to the other conclusion, that they originate within the body, and this seems to be the opinion of the best physiologists and pathologists. This doctrine appears to be almost brought to a demonstration by the following facts. First, it appears that the worms which have been found in man and animals have a peculiar structure and organization, differing materially from that of the worms which inhabit the external world. This is a point admitted by almost every modern writer on natural history. In the next place we find that the worms of certain animals present peculiarities differing from those of the same species in others. Thus the bothriocephalus and tænia solium in man differ from those of other animals. You are not, however, to conclude from this that every animal has its peculiar worms, for such is not the case. Thus the lumbrici and small ascarides of man are found to exist in various animals, both carniverous and graminiverous.

It appears obvious, that if worms were introduced from without, we should not find peculiar worms in the bodies of certain animals, yet taking a certain number of different animals living on the same food and in the same situation, we find a difference in the nature of the worms which are met with in the bodies of each. Another important fact is, that worms are to be found not only in the intestinal canal, but in almost every part of the body. We find them in the cellular tissue, in the liver, gall-bladder, lungs, and trachea; in the brain, heart, kidneys, and spleen. They have been met with in the air-bladders of fishes; and Treuter states that he has found the polystoma pinguicola in the ovaries of a woman which were steatomatous, and the strongylus in an aneurism of the mesenteric artery of the horse. These animals have been observed in the anterior chamber of the eye in birds and horses, and there are innumerable examples of their occurrence in situations equally strange and anomalous. Another circumstance already mentioned, and which must be coupled with the fact just alluded to is, that there are certain species of worms which occur only in the same organs, and are never met with in any other situation.

Now observe the importance of these facts — we find that worms not only exist in the digestive tube and parts having an external communication, but also in the very substance of deep-seated viscera, and that the worms which are found in the various cavities and organs are peculiar to them. In one case we find a worm in the digestive tube, in another in the brain, in a third in the liver, in a fourth in the pulmonary apparatus, but no one has ever been able to demonstrate the trajet of a worm from one of these cavities or organs to another. It would be ideal and absurd to say in the case of worms found in the substance of viscera, that they had been introduced from without or came from the intestinal canal. The distoma hepaticus, which is found in the liver and gall-bladder, might be supposed to arrive at those situations by passing along the ductus communis choledochus, but in the various cases in which it has been found, it has never been detected in the intestinal canal, and this, I think, would not have been the case if the digestive tube had been its original situation. One of the most important facts which have been stated is, that certain forms of these animals are found invariably in certain situations, and this has been observed not only in man and other animals of the class mammalia, but also in reptiles and fishes.

In man we generally find the lumbricus inhabiting the stomach and small intestine, the tricocephalus in the cœcum, and the small oxyrus, or thread-worm, in the rectum. The preparation before me exhibits a specimen of the rarest form of worms which inhabit the intestinal canal, the tricocephalus. Here is the cœcum filled with these singular worms. The males are distinguished from the females by the whirl of the tail. If these little animals or the oxyuris had been introduced from without, we should expect to find them in various parts of the intestinal canal, but we find, on the contrary, that their situation is separate and distinct.

Lastly, *intestinal worms have been found in the fœtus both of man and other animals.* Kerking describes a fœtus, the intestinal canal of which contained a vast quantity of small worms, and another of six months, in whose stomach a large lumbricus was found. Rudolphi, Blumenbach, and others of nearly equal authority, have recorded abundance of examples of worms existing in the fœtuses of various quadrupeds, *and also in those of birds which had just broken the shell.* Those who are obstinately attached to the doctrine that worms are introduced from without, have gone so far as to assert that the ova of the worms have been transmitted at the moment of generation, a doctrine so absurd that it is unnecessary for me to enter into any refutation of it.

With respect, then, to the formation of worms in animals, we cannot help coming to the conclusion, that they are originally formed within the body, and that, in fact, there is an original generation of these animals, the result of one organization taking place within another,—the production, in fact, of a distinct being. This idea does not appear so difficult of conception when you recollect that circumstances analogous to it are extremely familiar, and of almost constant occurrence. There is not much more difficulty in conceiving the formation of a living worm within the body, than there is of conceiving the organization of a portion of lymph thrown out upon the surface of a serous membrane. What occurs in both cases is, that under the influence of the vital principle of the original animal, a portion of matter previously inorganic, assumes the properties of life, presents distinct traces of organization, vascularity, and sensibility. The only difference between them is, that in one case the organized mass remains adherent to the matrix, and in the other it is cast off, and forms a separate being. In the present state of our knowledge all speculation on the mechanism of the formation of worms must of necessity be nothing more than mere hypothesis. The idea which Bremser entertains on this subject is, that *intestinal worms* are formed by the presence of semi-assimilated nutritious matter in the digestive tube. Food taken into the system under ordinary circumstances, is converted into a substance fitted for the purposes of absorption and nutrition, but when the process is not perfected, it is not taken up by the absorbents, and is then, according to Bremser, converted into an animal substance. This appears to be but a crude idea, unsupported by any facts; and it would be more philosophical to say that we know nothing about the matter. Besides, worms occur in various parts of the body, as well as the digestive tube; and to suppose the presence of unassimilated matter in such situations would be only supposing an absurdity. Bremser brings forward, in support of this theory, that worms are of very frequent occurrence in cases where the assimilating powers are weak or deranged, and says that nothing is more common than to meet with an abundance of these animals in scrofulous

persons, in those who have great appetites and bad digestion, and in children laboring under disease of the mesenteric glands. On the other hand, there are abundant instances of worms existing without the slightest apparent injury to the general health. In certain countries almost all the inhabitants have worms. But I believe all that we can affirm on this subject is this,—that they are not introduced from without, and that they are formed within the body by a process, the nature of which is exceedingly obscured.—*London Med. and Surg. Jour. May*, 1834. —*Ibid.*

8. *Pathology of Intestinal Worms.* By Wm. Stokes, M. D.—Can we connect the formation of intestinal worms with any known pathological condition of the intestinal tube? This is a question of no ordinary importance, for if we were able to connect their formation with an inflammatory or any other state of the digestive tube, it would furnish us with a key to correct and successful treatment. The school of Broussais are of opinion that worms are the result of an acute or chronic inflammation of the gastro-intestinal surface. This doctrine is by no means supported by the evidence of facts, for it has been established *that worms are found to exist not only in connection with every possible pathological condition of the intestinal canal, but also where the tube presented the appearance of perfect health.* We cannot, then, safely affirm that intestinal worms are connected with an inflammatory or non-inflammatory condition of the digestive tube. Andral states that he has found them in all conditions of the intestine, whether red or pale, dry, or covered with mucus. They are most commonly, he says, enveloped in a quantity of mucus, and there is some redness in the place where they are lodged, but this appears to be rather the effect of their presence than the cause. I believe it to be the fact, that persons in excellent health, and with the intestinal canal in the normal state, may have worms. Dogs who are killed while in a state of apparently perfect health, are often found to have a large quantity of tape-worm in their intestines. It is idle and hypothetic to say, that the formation of worms depends upon an inflammatory or non-inflammatory, an asthenic or sthenic condition of the digestive tube; their formation is owing to some modification of the vital power, the nature of which is unknown. I again repeat, that nothing can be stronger against the supposition that worms depend upon inflammation, than the fact of their being observed in considerable quantities in healthy individuals.—*Ibid.* —*Ibid.*

9. *On the Supposed Power of Worms of Perforating the Intestines.* By Wm. Stokes, M. D.—This question, which is an interesting one in many points of view, has been lately the subject of medico-legal discussion, and therefore demands a share of our attention. Of the different kinds of intestinal worms, the only one which is supposed to be capable of perforating the coats of the digestive tube, and escaping into the peritoneum or some adjoining organ, is the lumbricus, which is remarkable for its vigor and for the sharp and pointed shape of its head and tail. Many of the most eminent pathologists of modern times, and among the rest Andral, Rudolphi, and Carswell, are of opinion that these worms are totally incapable of perforating the intestinal tunics. Andral states, that there is no well-authenticated instance of this occurrence on record; and Rudolphi declares that they have no apparatus

for effecting a passage through any continuous tissue. On the other side of the question, however, there are some curious facts and cases given, which, supposing the worms incapable of perforating, are very difficult to explain. Dr. Fischer, of Vienna, gives the case of a female, in whom the following circumstances were observed on dissection. Two circular orifices were found in the colon, communicating with the cavity of the peritoneum; in one of these openings a worm was discovered, one-half of which lay in the peritoneal sac, and the other in the intestine. No other worms were found in the digestive tube, but a second worm like the former was found in the peritoneum. Here we have a very remarkable coincidence of perforation of a portion of the gut, with the existence of one worm in the cavity of the peritoneum, and another of a similar description, as it would appear, in the act of making its way in the same direction. These circumstances, together with the existence of a double perforation, seem to be in favor of the idea, that the openings had been made by the corresponding worms. Another case is mentioned in the Elements of Pathological Anatomy, by Andral, and he quotes the case, not as one of perforation merely, but to show that the symptoms of effusion of matter into the peritoneum may, under certain circumstances, be nearly latent. The subject of this case, a young man laboring under phthisis, had a tumor near the umbilicus, which increased rapidly in size, and presented a distinct fluctuation. Soon afterwards the integuments gave way, and a large quantity of matter was discharged together with a lumbricus. During the progress of this disease there was some tympanitis, but little or no pain had been complained of. On dissection, there was a considerable number of worms, and a quantity of matter found in the peritoneum, and the perforation in the arch of the colon, corresponding with the extravasated matter. Bremser gives a very curious instance of this kind as occurring in a species of fish. In this case the fish died, and it would appear, says Bremser, that the worm finding some extraordinary change had taken place, was determined to take a peep and see what was the matter, for it had perforated not only the intestinal tube, but actually made a passage for itself through the whole body of the fish until it reached the water in which it had been lying. Here, finding that its world extended no further, it stopped, and began to make its way back again to its original situation by a new opening, so that when it was observed by Bremser the two ends were in the intestinal tube of the fish, and the middle portion external. This, however, does not resolve the question, as to whether lumbrici are capable of perforating the intestinal canal or not. My own impression on the subject is, that we have not as yet any distinct and unquestionable evidence of these worms being possessed of any perforating power; but it is a fact, that there are a great many cases on record of worms being discharged in considerable quantities from openings in the intestinal tube, and where it would appear that the openings had been formed, not so much by the action of the worms themselves as in consequence of their exciting an irritation in some portion of the intestine, followed by inflammation, ulceration, and escape of the contents of the tube into the peritoneum. There are many instances of this kind. An interesting case is mentioned of a female who was attacked with pain in the groin, followed by the appearance of a tumor, which she was directed to poultice by her medical attendant. After some time the integuments gave way, a quantity of matter was discharged, followed by a large lumbricus, and

during the progress of the case, about one hundred of these animals were discharged through the opening. This is a well-authenticated case. Another case is mentioned of a patient who had been subject to constipation and violent attacks of colic. A tumor began to appear in the right hypochondrium, followed by pointing and ulceration of the integuments and a discharge of matter. A number of worms (I believe twenty-four) were discharged through the opening, which remained pervious, and the patient lived for many years afterwards with an artificial anus. This case appears to be not an example of direct perforation from worms, but of the accumulation of a mass of these animals in a particular portion of the intestine, giving rise to irritation, which terminates in ulcerative absorption of its tunics and escape of its contents. Inflammation is set up in some part of the intestine, this goes on until the coats are all destroyed, and the matter and worms escape into the peritoneal cavity; but if adhesion should prevent this, an opening will be formed in some part of the integuments covering the belly. In both cases the opening is produced, not by an exertion of the worms, but by an ulcerative and vital process. In support of this view, it has been observed, that worms have come out through these apertures not head foremost; the centre portion appears first, and you can draw it out like a loop. Such cases as the foregoing, then, cannot be fairly given as cases of perforation from worms, but as cases in which these animals, acting somewhat like foreign bodies, produced irritation, inflammation, and ulcerative absorption. There is a very curious case on record, of a patient laboring under abscess of the liver, which burst externally, and a lumbricus was discharged with the matter. The patient died; and on dissection it was found that the cavity of the abscess had a communication with the stomach, through which it was conceived that the lumbricus had got into the liver.— *Ibid.*— *Ibid.*

10. *Worms which inhabit the Intestinal Canal in Man.* By Wm. Stokes, M. D.— The worms which inhabit the intestinal canal in man are the following:— first, the lumbricus, or common round worm; next, we have the tape-worm, of which two varieties have been described; thirdly, we have the very curious worm, of which there is a specimen before me,— it inhabits the cœcum, and is called tricocephalus; lastly, we have the thread-worm, to which the name of oxyuris vermicularis has been lately given. The lumbricus generally inhabits some portion of the small intestine, but is also frequently found in the stomach. Persons have often vomited them, and they have been known to have crept out by the mouth. They have been found also in the pharynx, œsophagus, and large intestine. There is an interesting case mentioned by Andral of a child, who, in a state of apparently good health, was suddenly seized with symptoms of suffocation, and died. On dissection, it was found that a large lumbricus, which had come up from the stomach, had, when it arrived at the glottis, turned into its orifice, and by irritating the larynx, produced spasmodic closure of that organ, and suffocation.

The lumbricus presents very marked appearances of an advanced state of development. The male has a peculiarly formed penis, the female has her generative organs well developed, and both have an extensive alimentary canal. The tricocephalus is about an inch in length, terminating in a point; the sexes are different, and the male is distinguished from the female by the circular whirl of his tail,— it is always

found in the cœcum. The small thread-worms, with which you are all acquainted, are almost exclusively found in the rectum. These worms are found in vast numbers in some children; and it is said that the quantities of them which are discharged by the West India negroes are extraordinary.

The tenia, or tape-worm, is generally found in the small intestine; but it has also been observed in the stomach, colon, and rectum. The length to which this animal sometimes attains is almost incredible. Bremser mentions a case in which a tape-worm 150 feet in length was discharged by stool. Another case is given, in which the tenia had the enormous length of 300 feet. I have myself seen a large wash-hand basin filled by a mass of tape-worm, discharged after a strong dose of castor oil and turpentine. Still more extraordinary instances are recorded. Thus, in the Copenhagen Transactions we read of a tape-worm 800 ells in length. But in all probability there has been an error in these measurements, and many worms have been taken for one. This is rendered probable by the fact observed by Robinus, who found in the body of a man who had before death discharged fragments of tape-worm, a tape-worm extending from the pylorus to within six inches of the anus. The length of this single worm was scarcely 30 feet. One interesting circumstance connected with this animal is, that it is inferior in its organization to every other species of worm. It appears to be merely a simple homogeneous, cellulo-gelatinous mass, without any division of sexes, and without a nervous system, or generative organs. It is said also to occur principally in persons whose powers of life are low; and if this be the case, as I believe it is in many instances, it furnishes us with a very curious and interesting fact. The other better developed kinds are found in persons of healthy, good constitutions; but the tape-worms, though sometimes met with in such persons, are generally found to occur in persons of low and weak diathesis. Here we see a curious connection between the product and the producing cause. —*Ibid.*—*Ibid.*

11. *Symptoms of Intestinal Worms.* By William Stokes, M. D.—It is a singular fact that we have not one single pathognomic sign of the existence of intestinal worms, except the circumstance of their being occasionally passed by stool, or vomited; almost all their symptoms are referable to irritation of the gastro-intestinal surface, and its sympathetic relations. Persons who are much subject to worms in these countries, are generally of a pale complexion, with a bluish circle round the eyes: the belly is more or less prominent, and there are various signs of irritation of the digestive tube, with itching at the nose and anus; head-ache; foul breath and tongue; irregular and sometimes canine appetite, nausea, hiccup, borborygmi, tenesmus, diarrhœa, and constipation. Though the patients take abundance of nutriment, they are generally thin and pale, and in such cases there is either one or two very large worms, or a great number of smaller ones, or their presence is complicated with disease of the intestinal canal. Such persons are also observed to be of an indolent and languid habit; they have perspirations, disturbed sleep, with grinding of the teeth, and irregularity of pulse.

The sympathetic irritations, produced by worms, are numerous and extraordinary. The genital organs may be excited, and we may have priapism and seminal emissions in the male, and irritation amounting

to nymphomania in the female. There is a very singular case on record of a female, aged 70, being seized with a violent attack of nymphomania from this cause. The nervous affections, produced by worms, are so Protean and so numerous, that it would be almost impossible to detail them; in fact, there is not a single nervous disorder which may not be simulated by the sympathetic irritation of worms. Epilepsy, hysteria, convulsions, dilatation of the pupil, amaurosis, symptoms of hydrocephalus, and even mania are among the affections of the nervous centres, or their immediate connections, which, in repeated instances, have been found to depend on the presence of worms. Kraus gives an extraordinary case of a man, who, at a very advanced age, became subject, from this cause, to fits of continued and inordinate laughter.

There is another case on record of convulsions depending on worms, which, like those from the bite of the tarantula, are said to have been soothed and relieved by music. Hufeland, in his journal, mentions a case of yellow vision from the same cause, and there are several instances of aphonia and mania on record, which have yielded to treatment which had removed intestinal worms. A case is mentioned of a person who got violent spasmodic action of the muscles of the eye, producing inversion of that organ to such a degree, that the eyeball appeared to be nothing more than a mass of red flesh. A case is recorded by Serres, in which the symptoms strongly resembled those of hydrophobia, and it is probable that some of the cases of hydrophobia, said to have been treated successfully, were nothing more than this extraordinary irritation of the nervous system produced by worms. I saw myself a case, in which two eminent physicians made the diagnosis of hydrocephalus; it was that of a child who was certainly to all appearance laboring under cerebral disease, for he had convulsions, coma, and dilated pupils. It was remarkable, however, in this case, that the treatment directed to the head, though early and well applied, proved totally inefficacious. A large dose of calomel was given, and some lumbrici passed; in the space of two or three hours there was an evident improvement, and the child quickly recovered.

During the course of practice I have met with several examples of affections of the respiratory organs, depending upon the irritation of worms. This affection has been long known. I recollect the case of a boy who was brought to me with an extraordinary affection of the chest. He was of a gross habit of body, of a flabby scrofulous appearance, and laboring under disease of the elbow-joint; but his chief complaint was, that he passed the night in great distress from incessant cough and wheezing. On examining the chest, I found the respiration healthy, and no other symptom of pulmonary derangement except very slight bronchitic rale. On expressing my opinion of the case to the mother, she said that he was easy during the day, but that his condition was very different at night. To ascertain the truth, I took the child into the hospital, and found that her statement was substantially correct; for from four o'clock in the afternoon until next morning, he was in a state of orthopnœa, with loud, ringing, incessant cough. During the rest of the day he was free from cough, and tolerably quiet. The case was treated with calomel and ipecacuanha, tartar emetic and other similar remedies, but the disease was rather exasperated than improved. The boy had swelled belly and constipation, and for this he was ordered to take a dose of turpentine and castor oil. He passed some worms with relief to the existing symptoms, and from the consideration

of this, and the failure of the treatment for bronchitis, we were determined to persevere in the use of anthelmintic medicines, and, for this purpose, put the child on syrup of cowhage, to be followed by castor oil draughts. He passed vast quantities of thread-worms in the course of a few days, and when they had been all removed, the cough disappeared altogether, but as long as any of them remained, the symptoms of pulmonary irritation continued. There could be no doubt that this was a case of intermittent bronchial irritation from worms, for their evacuation was immediately followed by a complete cessation of cough and dyspnœa. I have also, since the foregoing, met with many other instances of a similar description. A young girl came into the Meath Hospital with chronic bronchitis and some degree of hepatization at the lower part of the left lung. Having heard from her friends that she was extremely subject to worms, I determined to try what would result from the use of anthelmintic medicines, and put her on the syrup of cowhage with aloetic pills. Under this treatment the cough was quickly removed, and the lower portion of the lung recovered its permeability. Here, it was remarkable, that not only irritation of the bronchial mucus membrane, but even solidification of the lung, were cured by treatment calculated to remove worms. Mr. Ramsay, in his paper published in the Medico-Chirurgical Transactions, gives several cases of hæmoptysis from this cause. I think I have seen several cases of phthisis, where the original source of pulmonary irritation seemed to be the existence of intestinal worms.

Let me here, however, remind you, that we should be cautious in attributing too much to worms as the causes of morbid symptoms. There are several reasons why you should be on your guard in this respect, one of the most obvious of which is this,—it does not follow, in the first place, that the symptoms in any particular case are produced by worms, because the same cause, which may have predisposed to the formation of worms, may have produced the symptoms in question, and there may be merely a coincidence of worms and of these symptoms. Even if we look to the results of treatment, there is a great deal of doubt and difficulty. There are many cases on record which are described as cases of epilepsy from worms, and where all the symptoms have subsided under the use of anthelmintic medicines. In many of these cases we find the medicine chiefly employed has been oil of turpentine, and I need not tell you, that this is an excellent remedy in many cases of epilepsy totally uncomplicated with worms. The results of such cases do not necessarily prove that worms were the source of irritation. Again, immense injury is frequently done to children in persisting in the anthelmintic treatment for the supposed existence of worms. Recollect, the prominent phenomena of worms in the intestines are irritations of the digestive system and of other functions. Now, it is very well known that these symptoms may occur with or without worms. If, then, you have a case where these phenomena are present without the coexistence of worms, and if, under a mistaken impression, you treat it with anthelmintic medicines, you inflict a double injury; you exasperate the original disease by the drastic and irritating medicines which are ordinarily used for the removal of worms, and you do an indirect injury by neglecting to adopt proper means of treatment. There is nothing more common than to see children laboring under some irritation of the digestive tube, which is mistaken for worms, purged again and again until they get incurable enteritis or tabes mesenterica. When

a child has foul tongue and breath, picking of the nose, diarrhœa, and turbid urine, it is a common notion that he is laboring under worms. If he gets feverish, it is said to be the worm fever, and the anthelmintic treatment is pursued with unabated vigor. Now, I believe that a great majority of such cases are in reality disease of the mucous surface of the intestine, and that the consequent feverishness is dependent on this state. Another reason why you should be cautious is this,—in persons of an hypochondriac habit, there is nothing more injurious than their getting the idea that they have a worm in their bowels. When once this notion gets into the head of an hypochondriac, it is generally impossible to eradicate it. Some of the most melancholy and fixed cases of hypochondriacism are produced in this way; every symptom is attributed to the worm, the patient is in a state of constant feverish anxiety about it, he talks of nothing else, and is constantly taking medicines to expel it, to the great detriment of his general health, and with a manifest exacerbation of his symptoms. Medical men should be extremely cautious on this point. The patient is perhaps a female of hypochondriac and nervous habit; she has gnawing sensations about the epigastrium, which she supposes to depend upon the presence of a worm, and an injudicious practitioner favors the notion. He gives her various medicines to expel the worm; no worm is passed; she becomes more anxious, takes more medicine, and gets weak and emaciated. She then begins to think that all the nutritious matter in her body is going to support the worm, falls into a desponding state, and continues for the rest of her life an incurable hypochondriac.—*Ibid, May* 10, 1834.—*Ibid.*

13. *Causes of Worms.* By William Stokes, M. D.—Our knowledge is very scanty and inaccurate respecting the exciting causes of worms. The following, however, are generally looked upon as remote causes:—foul air, residence in damp and unhealthy situations, sedentary habits, and want of wholesome exercise, overfeeding, the constant use of certain articles of diet, as farinaceous substances, milk, cheese, sugar, etc. An eminent authority, (Bremser,) asserts, as I have already stated, that unabsorbed chyle in the digestive tube constitutes the most fertile source of worms. It is a common idea, that poor diet has a strong tendency to give rise to the formation of these animals, but it has been frequently observed that worms are met with in persons who are by no means in want of nourishment; and it is said that in cases where nutrition has been diminished in man and other animals, the worms die. If this be the case, it would appear that, so far from being the exciting causes of worms, poor diet rather tends to favor their removal. Uncooked vegetables and fruits are also reckoned among the causes of worms, but I believe this arises from the mistaken notion that the ova of intestinal worms occur in vegetables, and, being taken with them into the stomach, are there developed, or even changed in their organization, a position which we have already proved to have no foundation in truth. Persons who live principally on vegetable food have not been observed to labor under worms in a comparatively greater degree than those who use an animal diet. It is said that the Swiss, who consume a great deal of vegetables, are very subject to worms, but other nations who live in a similar way have not been remarkable for the same liability.

Worms have been stated to be occasionally epidemic. It is not very easy to determine this point, but it has been remarked, that at particular periods these animals have been more than usually frequent and numerous. Many authors have described an epidemic of what has been called *verminous fever*, that is to say, fever of a gastric or bilious character, accompanied by worms in quantity. It is hard to say what the nature of this fever really was, and whether it might or might not be fever with irritation of the digestive apparatus, one of the consequences of which was a discharge of worms already existing. That worms are endemic, is a proposition very easily conceived, for we see it illustrated by the extraordinary prevalence of these animals in sheep which are kept in low, damp pastures. In such situations worms are met with in great abundance in the liver and other parts of these animals.

It would appear, from the following remarkable case, detailed by Bremser, that the use of milk and farinaceous food predisposes to the formation of intestinal worms. This gentleman, who was physician to a monastery, and had ample opportunity of studying the habits of its inmates, was called to visit one of the oldest of the monks, who was said to be laboring under great derangement of the digestive system. On inquiry, he found that the patient had lived for sixty years in excellent health, using animal food, which, however, he had been latterly induced to change for farinaceous diet and milk. For a few days this agreed tolerably well with him, and then he began to be tormented with colicky pains, flatulence, sour eructations, and other distressing symptoms. His physician gave him some purgative medicine, and he passed a large quantity of tape-worm with relief; the treatment was persevered in, his former mode of living was resumed, and he recovered quickly. This case bears strongly against the fanciful hypothesis that the ova of worms are transmitted in the act of generation; for how could it be possible that the ovum of this tape-worm, transmitted in this manner, could remain undeveloped in the system for the space of sixty years! This case derives additional interest from the fact of a change to a farinaceous diet being apparently connected with the formation of worms.

Another remarkable case is given by the same author. The patient was a married female who had twelve children, — six boys and six girls. This woman observed, that whenever she was pregnant of a girl, she had a greater longing for milk and farinaceous food, and lived on those articles of diet almost exclusively. After living in this way for some time, she uniformly got an attack of worms, and this, as well as the longing for vegetables, coincided with the birth of a female child so invariably, that she was able to tell with certainty whether the child she carried was a male or a female. This is a singular and well authenticated fact. — *Ibid.* — *Ibid.*

13. *Treatment of Worms.* By Wm. Stokes, M. D. — The treatment of worms is, generally speaking, extremely simple, the principles of treatment in the various kinds of intestinal worms being nearly the same. Simple as they are, however, some persons entertain false notions respecting them. They appear to think that all they have to do is to evacuate the worms; and, having accomplished this, they rest satisfied, and take no steps to prevent their recurrence. But the mere

evacuation of worms is no proof of a cure; to effect this you must prevent their return. From what you have learned with respect to their exciting causes, you will be able to give such directions as to the patient's mode of living as will obviate their recurrence; and, with regard to the means to be adopted for removing them, we may divide them into the following:—We have, in the first place what is called the mechanical treatment, next the specific, and, lastly, the purgative treatment. The first and last are nearly connected. For instance, purgatives appear to act in the same way as mechanical anthelmintics, by irritating the mucous surface of the intestine and the worm, and thus causing its dislodgment and expulsion.

Among the principal mechanical anthelmintics are filings of tin, cowhage, powdered charcoal, and crude mercury. Among the specific are a variety of substances, most of which have a strong and peculiar smell. This is a very curious fact. Valerian, assafœtida, camphor, ether, and other odorous substances have been found to be anthelmintic, and the Geoffræa inermis, which has been employed for this purpose, is remarkable for its strong unpleasant odor. The same thing may be said of tobacco, the oil of chenopedium or wormseed, garlic, artemisia absinthium, and many others. With respect to purgatives, there is not one in the whole list, particularly those of the drastic kind, which may not be looked upon as an anthelmintic.

It is the opinion of the most eminent men, that the thread-worm is the most difficult to expel, because they are generated with an extraordinary rapidity, and accumulate in a very short space of time. You are satisfied of their existence, have seen them in the alvine discharges, and the patient has all the ordinary symptoms. Well, what is the best way of getting rid of them? You shall commence by the exhibition of a mercurial. It is difficult to explain why it is that mercury has such an effect in removing these worms, but the experience of the best practitioners can be adduced in proof of its efficacy. The statements of Dr. Latham, of London, and of many practitioners in this country and on the continent, go to prove this. In whatever way it acts, mercury appears to be a powerful anthelmintic; and it is a fact that these worms have been expelled where it was given in very small doses, and not sufficient to operate as a purgative. The best plan is, first to give a mercurial purgative, and then to have recourse to the mechanical treatment, giving with this view, the syrup of cowhage, one of the most efficacious of this class. It is a remedy which is easily managed, and will do no harm; for, though it produces violent itching when applied to the cutaneous surface, it produces very little sensible effect on the intestinal mucous membrane. The form which I employ is the following:—Take of the hairs of the dolichos pruriens one scruple, syrup of orange-peel an ounce; of this an electuary or syrup is to be made, of which you may give a child a tea-spoonful three times a day. This is the remedy on which the West India practitioners, who have frequently to treat this affection in the negroes, place the greatest reliance; and you will find that if you employ it, a vast number of worms will be often passed. It should be continued for two or three days, and then a purgative must be given, after the operation of which it may be again resumed, if necessary. An excellent adjuvant to this is the use of aloetic injections, composed of two parts of milk, and one of the decoction of aloes. In this way you will be able to remove a vast quantity of these

little animals from the rectum. It has also been observed, that injections of cold, fresh or salt water have a great power in promoting their expulsion. Bremser mentions that in cases where these worms pass from the rectum into the vagina in females, and excite irritation, there is nothing so effectual in destroying them as injections of cold water and vinegar. This you should bear in mind. You should also remember, in the case of administration of syrup of cowhage, to give strict orders not to let any of it drop on the child's skin, as it would excite a great deal of irritation. You should forewarn the attendants of its effects on the skin; and if any of it should be spilled on the hands, neck, or face, the best thing is to wipe and wash the part well, and then rub it with a little almond-oil.

For the expulsion of lumbrici there is nothing so successful as the ordinary purgative treatment. A bolus, composed of calomel, rhubarb, and jalap, will answer this purpose extremely well; you may also use the syrup of cowhage with much advantage. Bremser gives a formula for an electuary, which I have not tried, but have no doubt of its value, for it appears to combine all the qualities of a good vermifuge electuary. It is made as follows:—Take of the seeds of santonicum, and of the flowers and leaves of tansy, reduced to powder, each half an ounce. Here you have two anthelmintics of the specific kind. Add to these two drachms of powdered valerian: another. You then combine with these, two drachms of sulphate of potass, and a drachm and a half of jalap: these are purgatives. You then make them up into an electuary with syrup of squill, which is also an anthelmintic of the specific kind. Of this electuary two or three tea-spoonfuls are to be taken during the course of a day. Bremser states that this combination is of great value, particularly against lumbrici and tape-worm.

The treatment of tape-worm is not difficult. All the specific and mechanical anthelmintics are useful in promoting its expulsion, but there is nothing which appears to have such a powerful effect as full doses of turpentine and castor oil. This constitutes the best remedy we possess against the tænia; but if you wish to get rid of it entirely you must give the turpentine in full doses. You will frequently be astonished at the vast quantities of this worm which will be passed. When you give turpentine, it is safer to order a full dose of it, for if it be given in small quantities it is very apt to irritate the urinary organs. Half an ounce of turpentine, with the same quantity of castor oil, form an efficacious, though very disagreeable draught. You may, however, obviate its nauseousness by the addition of a small quantity of camphorated tincture of opium and mucilage of gum Arabic. The celebrated empyreumatic oil of Chabert is, in my mind, nothing more than a modification of the turpentine. This is the remedy which Bremser looks upon as most efficacious against the tape-worm. You have all, I presume, heard of the animal oil of Dippel—the oil which is produced by the distillation of bones or hartshorn shavings. To one part of this are added three parts of turpentine: these are left to combine for four days, and then distilled; the first three parts of oil which come over are called the empyreumatic oil of Chabert. It is an exceedingly nauseous remedy, has a most disgusting smell, and is seldom used in this country. Bremser recommends it to be taken in doses of a tea-spoonful three times a day. Some persons who have tried it have assured me that it is extremely difficult to be taken, and that it excites a train of most disagreeable abdominal sensations. Bremser, however, thinks highly of it; he is in the habit of directing his patients to take it for three or four

successive days, then to omit for a day or two, and then to return to it again, and he says that it not only succeeds in evacuating the worm, but also in preventing its return. In addition to this, he recommends the use of a fortifying tincture, which I think very useful in worm cases. It is a combination of one of the salts of iron, with a preparation of aloes. If you take equal parts of the muriated tincture of iron and tincture of aloes you will have a remedy somewhat similar to the strengthening tincture of Bremser. Twenty drops of this mixture, taken three or four times a day, will prevent the recurrence of worms. —*Ibid, of May* 3, 1834.—*Ibid.*

14. *Diagnosis of Inflammation of the Spinal Dura Mater.* By Professor ALBERS, of Bonn.--From two cases observed by himself, as well as analogous cases related by other practitioners, Professor Albers is of opinion that inflammation of the spinal dura mater may be distinguished by characteristic symptoms from inflammation of the spinal arachnoid and spinal marrow, with which it has hitherto been confounded. These symptoms are, 1st, acute pain extending to the lower limbs and lower parts of the body; 2d, convulsive motions of different parts; 3d, tremors; 4th, difficulty of expelling urine and fæces; 5th, sensation of constriction around the body. The professor explains these different phenomena in the following manner:--

1. The *acute pain* in the parts situated below the seat of the inflammation was noticed in the two cases reported by the author, as well as in the first observations of M. Funk. This symptom has been repeatedly noticed in cases of tetanus. The pain is usually accompanied with a sense of pricking and tearing; and these distressing feelings are in most instances referred to the epigastrium, hips, and thighs. In very severe cases so acutely sensitive may be certain parts that even the slightest touch causes excruciating pain and cramps. Now this symptom, or rather chain of symptoms, is not observed when the archnoid and pia mater, or when the substance of the medulla itself is affected: under these circumstances, after the pain and convulsive movements have continued for some time, a paralytic weakness usually supervenes. On the contrary, paralysis is certainly not a common result or effect of inflammation of the dura mater, unless the disease has involved the parts situated within its sheath.

2. *Convulsive movements.*—When the cervical portion of the spinal dura mater is affected, the muscles of the face and neck are usually drawn into irregular contractions, along with those of the chest and abdomen. The upper extremities are affected more tardily, and sometimes not at all, until a few days before death. It is not, however, unfrequent that severe and irregularly returning pains are felt from the shoulders down the arms; certainly the upper extremities are more generally affected than the lower, when the disease is situated in the cervical region. When the whole extent of the cervical section of the medulla is inflamed, the upper extremities are almost certainly affected; but when the inflammation is limited to its inferior portion, they may escape altogether, or exhibit only a trembling movement or agitation, while the lower limbs are all the time contracted with tetanic spasms. Sometimes the arms are spasmodically affected, while the thighs and legs are paralysed. Under these circumstances, we may expect to find on dissection an effusion of reddish serum within the sheath of the dura mater, at its lower part. As long as the quantity of this

serum is small, it may act as an irritant on the cauda equina, and may thus cause convulsions of the inferior extremities; but when it becomes more abundant, it induces, in consequence of the compression, partial or total paralysis.

Professor Albers remarks, that the tetanic convulsions which attend inflammation of the dura mater are almost always of the "tonic," and very rarely of the "clonic" kind. The cause of the persistence and intensity of the spasm is no doubt attributable to the irritation which the diseased envelope exercises on the contained medulla, being constant and not remitting. "Now, this is not the case when the medulla itself or its immediate coverings are affected. The state of high excitement lasts but for a short time; hence the violent convulsions are either from the exhaustion of the nervous energy, or from the compression on the nervous mass, speedily followed by paralytic weakness. On the other hand, this state of paralysis seldom occurs in inflammation of the dura mater, except for a few hours before death."

3. *Tremulous agitation of different parts of the body.*—This symptom is usually most conspicuous in the early stages of the disease, when the patient attempts to walk. He finds that he cannot keep his head steady for a moment, and that his arms, body, and legs are in a constant tremor. This is one sort of the "paralysis tremens" of Dr. Cooke. It may be observed, that the involuntary action of the muscles we are now alluding to, always ceases when the patient is in the horizontal position. As the disease lasts, this symptom becomes less and less distinctly marked, and either disappears altogether, when a favorable termination may be anticipated, or it is succeeded by a tetanic stiffness or contraction of the limbs. As soon as tetanus begins to appear, the tremulous agitation ceases.

4. *Difficulty in expelling the urinary and alvine evacuations.*—In general, the paralytic affection of the bladder precedes that of the rectum; and, indeed, the latter viscus is sometimes but little affected till within a short time before death. The true nature of the urinary local distress is not unfrequently mistaken at first, and it is only when the bladder has become enormously distended, that the practitioner is made aware of the existing evil.

5. *The feeling of constriction round the body.*—When the cervical portion of the medulla is inflamed, the sense of constriction is usually experienced round the lower part of the thorax, extending from the dorsal vertebræ to the epigastric region. When the lower part of the dorsal, or when the lumber division is the seat of the disease, this symptom is remarkable chiefly in a line, running almost parallel to the anterior and superior crista of the os ilii. The sense of constriction is not unfrequently accompanied with positive pain; it is always an unfavorable sign, as it indicates the intensity of the inflammatory action. When once established, it is, even under the most fortunate circumstances, very obstinate of removal.

In concluding these remarks on inflammation of the spinal dura mater, we may state that the functions and faculties of the brain may remain uninjured, under the most serious disease of the medulla spinalis. —*Archiv. Generales, Sept.* 1834, *from Græfe and Walter's Journal, XIX.* 3.—*Ibid.*

15. *Of the Influence of Professions upon Phthisis Pulmonalis.* By M. Lombard, of Geneva.—In the list of the professions which dispose

to phthisis, all are enumerated with the exception of three or four, and these have been noticed as preventing the disease, viz: butchers, coal miners, fishmongers, and tanners. The author of this memoir, M. Lombard, of Geneva, judges that this list might be greatly enlarged. At first, by analyzing the bills of mortality collected in the hospitals of several large cities, and the observations collected in the official records of Geneva, he classes the professions into two series, according as they number more or less than 114 phthisical cases in 1000 deaths. After many calculations, intended to expose apparent contradictions, furnished by the statistics of different countries, and of the various trades, he concludes that the following professions have been improperly classed among those which dispose to phthisis. On the contrary, they appear rather to be opposed to its development: lawyers, dyers, founders, stocking-makers, bleachers, bakers, quarry-men, chandlers, brasiers, gilders, blacksmiths, miners, porters, locksmiths, and wharf-porters. Passing to examination of the causes which produce these results, M. Lombard at first notices the state of ease or poverty which the different professions imply or suppose, and he concludes that the poorer classes are ten times more accessible to phthisis than those who are situated in ease and comfort. He then passes to muscular exercises. A sedentary life causes a much greater number of cases of phthisis than an active life. The sedentary professions give as a mean number of phthisical cases, 141 in 1000 deaths. Whereas the active professions, in the same number of deaths, only give 89 cases of phthisis. The exercise of the voice is not, notwithstanding the general opinion, a circumstance which assists the development of phthisis. If there are some persons who experience the bad effects of the constant exercise of the voice, this exercise is, in general more advantageous than injurious. The professions which exercise the voice, only afford 75 cases of phthisis in 1000 deaths. With regard to the bent position, it is necessary to distinguish the trades which force this position whilst great muscular exercise is employed: hatters, tanners, gardners, wood-sawyers, and the trades which in bending the body leave it in almost complete repose: tailors, engravers, watch-makers, shoe-makers. The latter afford a mean of 134 cases of phthisis in 1000 deaths; the former only 83. Hence, although they might induce us to consider the constraint occasioned in the functions of the lungs, as a frequent cause of phthisis, this circumstance appears to exert but a secondary influence, and is more than counterbalanced by constant muscular exercise.

Influence of the purity or impurity of the atmosphere. In the professions exercised in the open air, 75 cases of phthisis in 1000 deaths; in professions exercised in factories, 138. When the factories are large and open, phthisis is less frequent than in small and close factories. The healthfulness of the atmosphere which surrounds factories is often altered by foreign particles, the contact of which with the lungs have a remarkable influence upon the development of phthisis. Foreign particles may be dissolved in the atmosphere or simply suspended in the air. Aqueous vapors. All professions which are exercised in the midst of these vapors, as tanners, bleachers, water-men, water-carriers, laundresses, are classed below the mean, 114. The uniformity of this result is as remarkable, as the theory would lead us to a contrary opinion. Observation shows, that cold and moist climates are those in which phthisis exercises its greatest ravages. And yet the occupations in which the workmen which are surrounded by watery emanations only

give 53 phthisical cases in 1000 deaths. This conclusion, drawn from observations at Geneva, is opposed to that which M. Benoiston de Chatauneuf has drawn from observations at Paris. According to M. Lombard, the bleachers of Paris are exposed to many causes of phthisis independent of mere moisture of the atmosphere. A dry and warm air appears to be an active cause of phthisis pulmonalis. The proportion of phthisical cases in 1000 deaths, of iron-mongers, jewellers, makers of files, founders, forgers, is about 127. The frequency of phthisis with other workmen, as watch-makers, toy-men, mounters of watch-cases, goldsmiths, depends, without doubt, upon the high temperature of the stoves which they use, a temperature which dries and purifies the air of their workshops. The influence of animal emanation, appears to be as advantageous as that of a moist atmosphere, among butchers, tanners, chandlers, nurses of the sick, butcher's wives. The proportion of phthisical cases in 1000 deaths is only 60. An atmosphere which is charged with the emanations of living plants may pass as a preservative from phthisis; gardners are less subject to it than farmers; in 100 deaths of gardners only 4 were owing to phthisis. This is not the fact with regard to emanations from dead or fermenting vegetable matter: attendants on wine cellars and bakers are very subject to phthisis. The number of phthisical cases among starch-makers is less than the mean. Among varnishers the proportion of phthisical cases is considerable: in 65 deaths of varnishers of paintings at Geneva, 24 succumbed to phthisis. Picture painters, although exposed to emanations of turpentine and the drying oils, succumb less frequently than varnishers, because they are not obliged, as these latter, to saturate themselves, as it were, with noxious emanations, by closing so carefully their work shops in order to prevent the dust from attaching itself to the varnish. Emanations from the mineral acids do not appear to be deleterious. The nitric acid is used by hatters, gilders, assayers, goldsmiths. Of these four occupations but one is above the mean, 114, the rest count very few phthisical cases. Phthisis is also rare with those who are engaged in making chlorine. As to the metallic emanations, the statistics of Geneva are opposed to those of Paris, collected by M. Benoiston; and M. Lombard suspends his opinion until there is more ample information. There is the same uncertainty as regards the vapors from lead, arsenic, antimony, and copper; at the same time he is inclined to think that these vapors are less prejudicial than is commonly supposed. Those occupations which expose the workmen to an atmosphere charged with foreign particles, may be divided into two classes, according as the molecules are large or very much divided. In the first class the proportion of phthisical cases in 1000 deaths is about 137, in the second about 152. The mineral dusts cause 177 in 1000 deaths; vegetable, 105; animal, 144. Then, in fact, the most injurious of all foreign bodies which can exist in the atmosphere is the impalpable dust of very hard substances. Moreover, the mineral dusts are the most deleterious, particularly those of steel and silica. Dr. Knight, of Sheffield, has remarked, that there is not, at that place, a single polisher of steel forks who has attained his thirty-first year. This shortness of life is remarkable with the cutters of flints, freestone, and crystals. The filamentous dust of cotton, wool, feathers, etc. is more injurious than that of flour or starch. We will now recapitulate the professions in order as they exert an injurious or preservative influence. Mineral and vegetable emanations, 176 cases of phthisis in 1000 deaths; various dusts,

145; sedentary life, 140; a life passed in manufactories, 138; warm and dry air, 127; bent postures, 122; movements of the arms causing shocks to the chest, 116. Let us recollect that the mean of phthisical cases, among all kinds of diseases, is 114 in 1000 deaths. Now let us enumerate those that have a preservative effect: an active life, with muscular exercise, 89 cases of phthisis in 1000 deaths; exercise of the voice, 75; a life passed in the open air, 73; animal emanations, 60; aqueous vapors, 53. M. Lombard devotes the latter part of his paper to the therapeutic applications, which there is no necessity of repeating here, they may be induced very naturally from the facts detailed. We will only note the kind of climate which he advises physicians to recommend to their phthisical patients. This climate ought to be moist, at the same time warm and serene. He also prefers Rome and Pisa to Nice, to Naples, and particularly to the dry and brisk climate of Marseilles and of Montpelier. — *Gaz. Med. May 25th*, 1834, *from Annales d'Hygiene et de Med. Leg. Jan.* 1834. — *Ibid.*

16. *On the Probable Duration of Life among Medical Practitioners.* — By Professor Casper, of Berlin. There is no part of medical statistics more painfully interesting, and at the same time important, than that which relates to the probabilities of the duration of life. Inquiry on the subject, it is true, scarcely tends to lift the thick veil which conceals the limits of individual life, and has little power to satisfy the pardonable curiosity which we all have, to know how long we may have to live: yet, when applied to masses, it enables us to arrive at results of great consequence, whenever provision is to be made for the duration of life in general, or for the probable duration of it at certain epochs.

For upwards of ten years I have been engaged, during my leisure moments, in researches of this kind — the difficulties of which can only be compensated by the value attaching to each result. I have considered most of the circumstances which tend to modify the duration of human life, and those more particularly which are connected with the practice of the different professions. Much has been attempted within the last two hundred years on this subject by mathematical and statistical enquirers, yet much remains to be done; for, in fact, with the exception of Deparciex's work on the mean duration of life among monastic persons of both sexes, we cannot be satisfied with any thing that has hitherto been accomplished.

Although I should not feel justified in laying at present the whole of my work before the public, there is, notwithstanding a portion of it with the correctness of which I am so well satisfied as not to hesitate in publishing it: the portion or fragment to which I allude, relates to the probabilities of the duration of life among medical men.

Establishments having a close connection with civilization, such as life insurance companies, those for annuities to survivors, for mutual insurance, etc. have become numerous of late years; and the institution, set on foot by the illustrious Hufeland, for the benefit of members of the profession, may serve as a model of the kind. But none of these establishments, as the experienced can testify, and as lamentable events have too clearly shown, can possibly subsist for any lengthened period, unless founded on very exact tables of mortality. It was with this impression that the following tabular view was formed.

In order to obtain the most satisfactory points of comparison, I have noted down 624 cases of death, occurring among physicians and surgeons, mostly all Germans,—and excepting anatomists, veterinarians, naturalists, and medical men, who are solely devoted to literature.* I have taken the age of twenty-three as my *point de depart*, both for the medical as well as the other professions, (a comparison of which it is my intention hereafter to make public;) but if others should choose twenty-four or twenty five as the age from which we should begin to calculate, it in nowise interferes with the exactitude of the results.

My chief authorities in ascertaining the dates of the births and deaths of those on whom I reckoned, were Ersch, in his Manual of Literature, (Leipsic, 1822,) and Voigt, in his Necrology, (1833,) whose valuable collection is known to have been principally formed from manuscripts communicated by the friends and relatives of the deceased.

In column A of the following table we have the ages at which our medical men died: B contains the number of deaths at those ages.

Now we suppose the 624 individuals on whose deaths the table is constructed to have been contemporaries, or that they were all aged 23 at the same time. On this supposition the cypher 624 is placed in column C opposite the age of 23; and as there are two deaths for that age, 622 is the cypher which must stand next in column C, corresponding with the age of 24 in column A. Thus the column C is formed throughout, until the whole of the 624 are deceased, which is found to be at the 92d year. Column D exhibits the number of years which may be allotted to each period—that is, the probable duration of life according to Halley's method.† I have preferred the form which Halley follows to that of Deparcieux, as being less complicated, at the same time that it leads to the same results. By Halley's method, we find the probable duration of life very readily: we look into the column C to ascertain at what age only half those living at any given period survive. Thus, if of 122 medical men who are 72 years of age, we find that the half are dead at 77, it would seem that the chances of the 122 reaching that age, and of not reaching, it are equal; and the difference of the ages, (77—72)—nearly five years—is the length of time which those of 72 may hope to live.‡ Nothing positive, of course, can be predicated from the table respecting the probable duration of the life of any individual; but the general conclusions, as experience has proved, are nevertheless *extremely certain*.

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*I have since calculated the mean duration of the lives of seventy-six other medical men—thus making up the entire number 700; but I have not thought it necessary to enlarge my table by the addition of the results, which only serve to confirm those already obtained.

†Philosophical Transactions for 1691 and 1693.

‡In general terms: if m be the number of persons who have attained the age a, and that at the age n the number is found to be reduced to $\frac{1}{2}m$, n will express the probability of vital duration, and $n-a$ the number of years on which the individuals may calculate.

Table of the Mortality of Medical Men.

A. Age.	B. Deaths.	C. Living.	D. Probable Life.	A. Age.	B. Deaths.	C. Living.	D. Probable Life.
Years.				Years.			
23	2	624	35, 5	58	10	317	11, 0
24	1	622	34, 4	59	17	307	10, 6
25	4	621	35, 4	60	12	290	10, 3
26	3	617	33, 0	61	15	278	9, 7
27	7	614	32, 0	62	14	263	9, 0
28	5	607	31, 4	63	19	249	8, 8
29	5	602	30, 1	64	20	230	8, 5
30	5	597	29, 8	65	11	210	8, 0
31	11	592	28, 6	66	18	199	7, 5
32	8	581	28, 0	67	6	181	7, 2
33	11	573	27, 6	68	16	175	6, 5
34	11	562	26, 8	69	9	159	6, 0
35	8	551	26, 0	70	17	150	5, 5
36	7	543	25, 5	71	11	133	5, 4
37	8	536	24, 7	72	15	122	5, 0
38	14	528	24, 0	73	14	107	5, 0
39	8	514	23, 5	74	13	93	4, 7
40	9	506	22, 7	75	10	80	4, 6
41	11	497	22, 0	76	9	70	4, 4
42	6	486	21, 3	77	8	61	3, 9
43	8	480	20, 3	78	10	53	3, 3
44	8	472	19, 7	79	4	43	3, 0
45	11	464	19, 0	80	11	39	3, 0
46	4	453	18, 2	81	6	28	4, 0
47	14	449	17, 3	82	3	22	4, 0
48	11	435	16, 7	83	3	19	4, 0
49	12	424	16, 0	84	2	16	4, 0
50	13	412	15, 4	85	3	14	4, 0
51	8	399	15, 0	86	2	11	3, 5
52	11	391	14, 2	87	—	9	2, 6
53	10	380	13, 5	88	2	9	2, 0
54	18	370	12, 8	89	4	7	1, 5
55	14	352	12, 6	90	1	3	0, 5
56	13	338	12, 4	91	2	2	0, 0
57	8	325	11, 9	92	—	0	——

From this table we gather the sad certainty of the short career of those who practice the medical art. Supposing the ordinary duration of human life to be 70 years, we see that scarcely a fourth part of our medical brethren attain that age; and hardly 1 in 15 reaches the age of 80. Or again: if in youth the resolution be taken to devote oneself actively to the laborious pursuits of medical science and practice, in order to enjoy in old age the fruits of a life of exertion, it is a melancholy fact, that one-half the number of practitioners is cut off before that period: so that the remarks of Jean Paul are peculiarly applicable to our case. "The life of man," says this author, "has often been compared to that of travellers or pilgrims: it seems to me rather to resemble that of an innkeeper, who is ever busy about his guests, receiving them, and seeing them depart; and who, on the occurrence of every interval of unprofitable repose, longs for a fresh bustle, desirous of more work when at rest, and of rest when at work; always hoping for the time to come when quiet and ease will suffer him to enjoy his arm-chair in tranquility; but, in general, before that time arrives, he is gathered to his eternal rest."

There are a few points of comparison between the mortality in the medical and that in other professions, which I cannot willingly omit to notice. Divines, (*les theologiens*,) among all the professions, are those who seem to stand the best chance of long life; and if *they* die off more slowly, or enjoy more longevity than the other, it is just the reverse with medical practitioners. The following summary will show the difference:—

AGES.	DEATHS.	
	Physicians.	Divines.
23 to 32	82	43
33 42	149	58
43 52	160	64
53 62	210	182
63 72	228	328
73 82	141	255
83 92	30	70
	1000	1000

And the following list may serve to show how much shorter the probable length of life is among medical men than among others. Taking 100 individuals in each calling, the number who attained the age of 70 have been, among

Divines,	42
Agriculturists and foresters,	40
Employes in high offices,	35
Mercantile persons and traders,	35
Military men,	32
Employes in lower departments,	32
Advocates,	29
Artists,	28
Teachers, Professors,	27
Medical practitioners,	24

But what are the causes to which we must attribute this low place in the scale of vitality held by our profession? It would be needless to enumerate them to those who are familiar with the extent of labor which the practice of medicine entails. There is, perhaps, no profession which requires more moral and physical exertion than ours, which allows less of repose, or of that tranquility which is so conducive both to internal and external life. There is none which exposes the members of it to such bodily fatigue, such mischief from bad weather—such disturbance of the night's rest,—so much watching, irregularity of meals, disorders of the digestive organs, and mental affections of all kinds: in a word, to such dangerous influences, perpetually recurring, and all tending to sap the vital powers. If we add to this; that there are far more practitioners cut off by contagion than are commonly supposed, we shall easily see the groundlessness of the satirical observation, that *sybaritism* is the rock on which medical men are wrecked. It may be mentioned, that in the table given above, the great majority of those who were the subjects of it were country practitioners, whom to accuse of luxurious living were too bitter a satire,—one, indeed, wholly undeserved by a profession, which has such strong claims to

public gratitude, since those who practice it abridge at least, if they do not absolutely sacrifice their existence, in endeavoring to prolong that of others.—*Medical Gaz., from Berliner Medicinische Zeitung, and Annales d'Hygiene.*—*Ibid.*

17. *Austrian Statistics.*—In the year 1833, the number of deaths in the Austrian monarchy was 665,731, being 76,917 fewer than in the preceding year; the deaths from cholera, however, in the latter year, may account for the difference. The number of births was 815,293. Among the deaths were by suicide, 724; hydrophobia, 35; casualities, 503; murdered, 422, (in the preceding year, 466;) executed, 36, (in the preceding year, 53.) There were 450 persons who were above 100 years of age. The population of Austria, including Lombardy, Venice, Dalmatia, the Tyrol, etc. is at present reckoned at about 34,000,000. —*London Medical Gazette, Jan.* 1835.—*American Jour. Med. Sci. May*, 1835.

18. *Method of Obtaining the Separation of a Necrosed Sequestrum without an Operation.*—It is stated by Dr. Bouget, that as early as 1814, Professor Delpech, having been disappointed in the treatment of some cases of necrosis, resorted to the plan of dissolving the calcarious matter of the bone by the application of sulphuric acid, in order to soften its texture sufficiently to render it susceptible of being cut with a knife or cutting forceps. A young man whose arm had been amputated, experienced two very severe attacks of hospital gangrene, which occasioned the end of the bone to project an inch and a half beyond the stump. Months probably would have been consumed in the separation of this sequestrum; but this delay Professor Delpech effectually obviated. He had the external surface of the denuded bone covered with a pledget of lint dipped in a solution of diluted sulphuric acid. A ball of charpie, wet with the same fluid, was likewise introduced into the medullary canal, the reticulated texture of which had been previously broken down. In twenty-four hours the portion of bone was found sufficiently softened to admit of its being separated with ease, and in ten days the end of it was covered with healthy granulations, after which a cure was speedily accomplished.

In 1816, an individual presented himself at the Clinique at Montpellier, affected with a necrosis involving nearly the whole extent of the tibia. Although he was possessed of a good constitution, and was in a condition to undergo a formidable operation, M. Delpech resolved to adopt the procedure by which he had succeeded in the above case. He was desirous at the same time of accomplishing this object without the necessity of dividing the integuments by instruments. To effect this, an eschar was made with caustic potassa upon the upper part of the leg of the extent of six francs, and after this eschar was detached from the bone, a pledget of lint dipped in diluted sulphuric acid was applied to it, which, after two or three dressings, renewed at intervals of six hours, became so soft, that it could be broken down with common dressing forceps. Two succeeding applications of caustic potassa and sulphuric acid were then made lower down, in a line with the first, and in this manner the sequestrum was exposed to the extent of five or six inches in length by one and a half in breadth. It was extracted with great facility, measured more than six inches in length, and represented

nearly two-thirds of a cylinder. The patient experienced but very little pain, and was discharged cured in a month after his admission.

Dr Bouget represents that M. Delpech had pursued the same practice with equal success in several other cases, both in hospital and civil practice, and that he had himself resorted to it with great advantage in a case of necrosis of the tibia in a child.—*Jour. des Connais. Medicales, Jan.* 1835.—*N. A. Archives, Aug.* 1835.

19. *Nux Vomica in Spasmodic Asthma.*—A case is reported in Kausch's *Geist und kritik. der medec. chirurg. Zeitschriften*, of a young man, aged 20 years, who, after having employed all the most efficacious remedies for spasmodic asthma without success, was effectually relieved by the continued use of nux vomica. The habitual difficulty of respiration subsided, and the paroxysms of asthma disappeared and did not return.—*Strohmayer Medecinish. Prak. Darstellung*, 1831.—*Ibid.*

20. *New Method of Obliterating Aneurismal Tumors of the Extremities.* By M. Leroy D'Etoille.—The procedure proposed consists in establishing mediate compression upon two points of the artery, at an interval of two inches from each other. The portion of blood thus isolated, as it were, coagulates more promptly, than when compression is made in the ordinary way upon a single point. In order to promote the formation of a coagulum, M. Leroy keeps the part covered with ice, and with the view of coagulating the albumen of the isolated portion of blood, he employs acupuncture and galvanism.—*Lancette Francaise, Mars*, 1835.—*Ibid.*

21. *Lepra Vulgaris.*—The editor of the London Medical and Surgical Journal remarks:—we have lately cured a young woman of lepra vulgaris, of five years standing, which covered the whole cutaneous surface, and defied a variety of remedies, with the following ointment. Her general health was duly attended to.

℞ Hydriodate of potass, ʒ iss.
Prepared lard, ʒ iss.
Tincture of opium, ʒ i.

M. Gibert found the following successful at the Hospital St. Louis:

℞ Ioduret of Ammonia, ℈ i.—ʒ i.
Prepared lard, ʒ i.

Daily for twenty-nine days.—*Ibid.*

22. *Remedy for Itch.*—Dr. Hospital uses the following ointment with success:

℞ Lac. Sulph. ʒ iss.
Pulv. Clor. Calcis. ʒ ii.
Adipis, ʒ vi.

Double this quantity effects a cure.—*London Med. and Surg. Jour. May*, 1835.—*Ibid.*

23. *Cure of Jaundice.*—Frere Le Ome used to cure jaundice by giving a drachm of dried and powdered walnut leaves, infused in a glass of white wine every morning, fasting. From fifteen to twenty doses were for the most part sufficient to annihilate the disorder.—*Ibid.*—*Ibid.*

24. *Newly discovered arrangement of the Arteries in the Erectile Tissue of the Penis.* By Professor J. Muller, of Berlin. (*Archiv fur Anatomie und Physiologie*, Number II. for 1835.) The second number of Muller's (late Meckel's) Archiv contains the account of an interesting discovery recently made by the editor, Professor J. Muller, of Berlin, relating to the disposition of the small arteries of the penis, which tends to explain the structure of the erectile tissue of that organ, a subject which, notwithstanding the investigations of Cuvier, Tiedemann, Moreschi, and Panizza, was still involved in much obscurity. These observations point out a fact entirely new in the structure of the arteries of the erectile texture, and promise to throw some light on the nature of the erectile condition of the blood vessels.

Most of those who have investigated the structure of the erectile textures of the penis by injections, have contented themselves with filling the veins of the organ, and thus, although the structure of the venous caverns of the *corpora cavernosa*, and the dilated veins of the *corpus spongiosum urethræ* was sufficiently well understood, yet very little was known respecting the mode of termination of the smaller arteries. It has been generally believed that the same small capillary arteries which nourish the penis carry the blood into capillary veins, that the blood passes from these into the dilated venous branches or sinuses, and that the state of erection depends on the retardation of the flow of blood in these venous spaces.

Professor Muller has, by a careful injection of the arteries of the penis, pointed out, besides the capillary branches which nourish the penis and transmit the blood into the capillary vessels and dilated veins, a number of very remarkable appendices connected with smaller arteries, both in the *corpora cavernosa penis* and *corpus spongiosum urethræ*, which, from several circumstances, it is very probable, are the vessels more immediately concerned in maintaining an increased quantity of blood in the penis during erection.

The easiest way of rendering these two sets of arterial branches apparent, is to inject the principal artery of the penis, before its subdivision, with size and vermillion of moderate consistence, and then, making a longitudinal section of one of the *corpora cavernosa*, to wash away any part of the injected mass which may have passed into the venous spaces. The ramification of the nutrient arteries will then be easily seen upon the inner sides of the venous spaces, the arteries becoming smaller and smaller, until at last they pass into the minute capillary network, the branches of which cannot be seen with the naked eye. Besides these nutrient ramified arteries, there will also be seen upon a careful examination another set of arterial branches of a different size, form and disposition, which are given off nearly at right angles from both the larger and smaller trunks of arteries. These arterial processes are about one-hundredth of an inch in diameter, and one-twelfth long, and are quite easily seen with the naked eye. They project into the cavities of the spongy substance, and terminate either bluntly or by dilated extremities, without undergoing any ramification. These short arterial processes are turned round at their extremities into a semi-circle or more, and present a contorted appearance, which circumstances has suggested to Professor Muller the name of Helicine arteries, which he has applied to them.

The helicine arteries of the penis are more easily seen in man than in any other animal which Professor Muller has examined. He has

found them in all the animals in which he has sought for them; they are to be seen at the posterior part only of the penis in the stallion, but in the dog exist throughout the whole organ.

In man, the helicine twigs of the penal arteries sometimes come off singly, and at other times they form tufts or bunches, consisting of from three to ten branches, and having in general a very short common stem. The swelling at the extremity, when it occurs, is gradual, and is greatest a little way from the end. The helicine branches given off from large arteries are not of a greater size than those coming from smaller ones, and even the smallest capillary arteries of the *profunda penis*, which can be seen with the help of a glass only, give off helicine twigs of a much greater size than themselves.

Each helicine branch projecting into a venous cavern is covered by a thin membrane which Professor Muller regards as the inner coat of the dilated vein, and when there is a tuft of helicine twigs, the whole tuft is covered with one envelope of a gauze-like membrane. This covering is considerably thicker on the helicine arteries in the posterior part of the *corpus spongiosum urethræ* than in the *corpus cavernosum*, but it seems probable that this is in some measure connected with the state of repletion of the arteries, for when the injection has run very well, it becomes difficult to distinguish the external covering.

Professor Muller states, that he could not discover any apertures either in the sides or in the ends of the helicine arteries, but he seems to regard it as probable that minute apertures do exist, which may be of a nature to allow the passage of blood in some states and not in others.

The helicine arteries are not, as some might be inclined to suppose, loops of vessel which have been incompletely filled, and which, after making a coil, pass into venous spaces, as E. H. Weber discovered to be the case with the arteries of the maternal portion of the placenta; they are merely projecting branches from the arterial trunks containing blood.

The helicine arteries are more numerous towards the root than near the point of the penis. They exist in the *corpus spongiosum urethræ* especially towards its bulb, but they are not so easily seen there as in the *corpora cavernosa*. They have not yet been observed in the glans. Their structure is nearly the same in all the animals in which they have been observed; those of the ape bear the nearest resemblance to those of man, and in most animals they are less obvious than in the human subject. In the horse and dog, they give off small nutrient twigs from their sides, which render them more difficult to be seen in these animals than in man.

Professor Muller conceives these helicine of tendril-like arteries to have an intimate connection with the process of erection, and there is every probability that this is the case; but experiments and observations are still wanting to show in what manner these arterial branches are affected in the erected and non-erected condition of the texture in which they exist.

We recommend to the attention of our readers this discovery of Professor Muller's, whose researches have already done a great deal for the advancement of physiological anatomy. The memoir is accompanied with a well executed engraving, containing numerous representations of the helicine arteries, which we regret that time has not enabled us to copy.—***Ed. Med. and Surg. Jour. July*, '35.**

25. *Abnormal Secretion of Milk from the Scrotum.*—The following very extraordinary case is contained in the inaugural thesis of M. F. KOLLER, the first thesis presented to the University of Zurich. There is perhaps nothing similar to it to be found in the annals of the science, if we expect a case somewhat analagous, cited in *Hufeland's Journal*, Vol. 54, No. 2. The milky nature of the fluid secreted, was put out of all doubt by microscopic observations and chemical analysis. It was found to contain all the characters of milk, and its production was evidently connected in some way or another with the exanthema and ulcer of the right lower extremity. The nature of the stools and urine was natural throughout the whole period during which the patient was under observation. The case is taken from the *Gazette Medical de Paris* of Saturday last, July 4:—

H. V., 21 years of age, had enjoyed good health up to the age of 15, when he began to complain of some feverish symptoms. A few days afterwards the left lower extremity presented an eruption of small, slightly elevated red spots, occasioning burning heat and pain in the skin: these symptoms soon disappeared, but the foot began to swell, and the lower extremity became tumefied and soon acquired double its natural volume. The general health of the patient was now in no way influenced by the disease. The tumefaction emptied itself from time to time by two small orifices below POUPART's ligament, giving exit to a white transparent vicid fluid, in greater or less quantity. As the tumour diminished the exanthema reappeared. The patient passed three years in this state, which was not altered by the various remedies employed. At this period the genital organs always little developed and extremely small, began to develope themselves in a sudden and extraordinary manner; the scrotum especially became very large; the external surface of this part was soon covered with a number of small vesicles, which opened, and discharged a great quantity of milky fluid: when the scrotum was compressed, a quantity of the same fluid issued through the two small pores we have noticed, below POUPART's ligament on the inner sides of the thigh. This secretion, though generally of a nature completely milky, sometimes presented an aspect slightly serous. The tumor continued to enlarge, although the secretion was not constant, periodically, every three or four weeks. There were now pains in the groin and lions, darting to the scrotum, and when the patient assumed the upright posture, about two or three pounds of the milky fluid came away. Since the commencement of the disease in the scrotum, the ulcer of the leg had closed, and the tumefaction of the extremity much diminished. On the 23d of August, 1833, the patient was admitted into the hospital at Zurich. The scrotum was now tumefied, and to the touch gave the sensation of a female breast; the skin of the penis was tumefied; the color of the parts was more deep than natural; the corpora cavernosa were small, and retracted towards the pubis; the testicles were also very small, and drawn up to the rings; the larynx was little prominent, but the voice was not feminine; the beard and the hairs on the pubis were delicate and few; the right extremity was a little larger than the left.

During the patient's stay at the hospital, the secretion of milk took place several times. Thus, about the 26th of August, the usual precursory symptoms appeared, and more than three pounds of milk were discharged. On the 30th, there was a new access of fever, and an appearance of the exanthema on the right thigh. On the 11th of Sep-

tember, a fresh discharge of about half a pint of fluid took place, not presenting the same white color or consistence of the former, and giving out a little the odor of semen. During the months of September and October, the tumor of the scrotum and the secretion of a milky fluid alternated frequently with the exanthema on the thigh; however, the tumefaction of the skin covering the genital organs, diminished remarkably, as well as the quantity of fluid discharged, which became more serous and troubled in its appearance, and finally disappeared.

26. *Remarks on the Use of Oiled Silk and Warm Water Dressing.* By Mr. Liston.—The plaister used by Mr. Liston to recent wounds, such as after amputations, etc., is composed of a spiritous solution of isinglass, spread over oiled silk; the solution is spread, when hot, on strips of the silk, and applied immediately. After amputations, the strips are used of considerable length, so as to give support to the flaps, and an interval is left between each, to allow the escape of matter, should adhesion not take place. Mr. Liston has used it for some years, and has found it to succeed in almost every case. It is easily applied, does not heat the limb, as the usual mode of dressing does, is easily removed in case of secondary hemorrhage, allows the escape of offensive matter, and prevents the fœtor attending the confinement of the discharge; often the wound does not require a second dressing, and the plaister frequently supersedes the necessity of a bandage. The use of warm water dressings, as employed by the same surgeon, consists simply in pledgets of lint, the size of the ulcer, dipped into warm water, and applied to the part, a piece of oiled silk a little larger being placed over it to prevent evaporation. It is found, in some cases, quite effective, without the assistance of any other remedy; occasionally some slight astringent, such as a weak solution of sulphate of zinc or copper is indicated.

Both dressings are cleaner and less painful than any other; the last does not require a sponge; indeed, there have been no sponges used in the hospital for some time, except during operations. Mr. Liston said he had dispensed with them at Edinburgh, as he had seen the indiscriminate use of the same sponge by the various patients, attended with the worst consequences. It was often the cause of the spreading of one description of ulcers, of a malignant character, through the wards of the hospital.

It is not for the medicinal use of the water, as stated by a late writer on the subject, that this dressing is applied, it is in place of a poultice. It is by no means a novelty: many surgeons in the army and navy have employed it for years with success.—*London Med. and Surg. Journal, June*, 1835.—*N. A. Archives, Sept.*

27. *Treatment of Recto-Vaginal Communication.* By Sir B. C. Brodie.—The communication between the vagina and rectum used to be one of the opprobriums of our art, the patient's life being rendered miserable, with little or no hopes of recovery. Of late, however, a simple and scientific method of relieving the patient in these cases has been contrived by Mr. Copeland, who has succeeded in curing several patients laboring under it, simply by dividing the sphincter muscle of the anus. The sphincter muscle being divided, the fæces are not retained in the rectum; they run out as fast as they enter it, so that the bowel is kept empty and contracted, and altogether in a passive state, and the

communication between the rectum and vagina is thus enabled to cicatrize. I do not know whether this would answer if the communication were of large size, but I am told that it has answered very well in the cases in which Mr. Copeland has hitherto employed it. I cannot but regard the application of this operation to these cases as one of the principal improvements of modern surgery; and the simplicity of the practice forms one of its principal recommendations. Of course it can be recommended in those cases only in which, independently of the opening into the vagina, the parts are in a healthy state.—*London Med. Gaz., June*, 1835.—*Ibid.*

28. *Camphorated Citrine Ointment in Hydrocele.*—A few weeks after a fall which occasioned an inguinal hernia, the right testicle, without any evident cause, began to swell, and became too painful to be handled. (The accident occurred to our correspondent.) This was soon followed by hydrocele, when the pain, which had by this time abated considerably, went off gradually, but the swelling seemed to be stationary. Meanwhile the hydrocele increased, and caused much suffering from tension of the scrotum. I consented to have the water drawn off, but in the interval a different mode of treatment occurred to my mind. Having frequently witnessed the beneficial effects of the citrine ointment, (*Unguentum Hydrargyri Nitratis*,) with camphor in cases of glandular and other indurated tumors, I thought it would also excite the torpid absorbents in those of an aqueous kind, and therefore preferred this trial to that of the knife. In applying it, I grasped the left or sound side of the scrotum firmly, so as to make the tumor as tense as possible, and then rubbed a small quantity of the ointment (about 25 or 30 grains) over its surface for a due length of time. This I repeated every evening as long as it was necessary, generally before a fire. After using the ointment for a week or ten days, the tumor was evidently diminished in bulk, and, persevering with it some time longer, the tumor eventually disappeared. In the meantime there was little alteration in the swelling of the testicle, but as it gave me little or no pain, except when handled, I left it to its own vis medicatrix, and that was slow enough, as it was fully three months before it was reduced to the size of the other. The hydrocele has never returned in any way. The citrine ointment was made by adding a drachm of camphor to an ounce of the ointment.—*London Lancet, May*, 1835.—*Ibid.*

29. *Catechu in Mercurial Pytalism.*—Capt. B., twenty-five years of age, having had occasion to undergo a short course of mercury, whilst his regiment was encamped near Harwich, during rather a cold summer, unexpectedly experienced, about the fifth day of the course, approaching pytalism; and when I was summoned to visit him in the evening, I found him in bed, spitting, or rather slavering, profusely, and presenting a picture of despair, from distress of mind, occasioned by his situation at a moment when he was daily expecting a visit from a party of valued friends, most of them females, to the camp. It was no easy matter to determine, under these urgent circumstances, what means to use. I had long ago proved the utter inefficiency of alum, opium, and other reputed antidotes against salivation, and at last I thought of catechu, of which I had made a strong decoction for some other purpose, though it was a remedy as yet untried, at least by me.

To be brief, I desired my patient to take a wine-glass full (about ʒii. to ʒiiss.) of this decoction every two hours, or even oftener, if his stomach would bear it; and also to gargle his mouth and fauces as frequently as possible with the same. The result was, that when I visited him early the next morning, the spitting had entirely ceased, and every other sign of the effects of mercury had wholly disappeared, debility from the pytalism alone excepted. I think that since this case occurred I have had further proof of the efficacy of catechu in obviating the ultimate effects of mercury, and I invite your numerous readers to make trial of it in cases of mercurial pytalism. Instead of ʒiiss. of catechu to half a pint of boiling water, according to the London Pharmacopœia, I use at least ʒiij. to that quantity of boiling water; for whether infusion or decoction be used, it ought to be made as strong as possible.—*Ibid.*—*Ibid.*

II.—ANALYTICAL.

I. *Diabetes Mellitus cured by Tincture of Cantharides.*—Professor Hall, of Baltimore, has published in the N. A. Archives for July last, the notes of a case of this obstinate disease, successfully treated with the tincture of cantharides. The patient was a youth of 17 years of age. The symptoms were not indicative of an inflammatory diathesis. Sulph. quin. phosph. ferri, etc. with animal diet were used without effect. He began by taking 20 drops of the tincture three times a day, gradually increasing the dose to 400, with but slight strangury! In about a month he was quite well.

II. *Ligature of the Brachial Artery for the Aneurism from Bloodletting.*—In the highly respectable periodical just quoted, we have a case, reported by the indefatigable professor Smith, of the University of Maryland, in which he resorted to this operation with success. Pressure above and upon the tumor, although it arrested the pulsation and emptied the sac did not effect a cure.

"The operation was performed in the presence of the medical class of the University of Maryland, and with the assistance of the pupils of the house. The patient was seated in a chair, and his arm was extended upon a table. The incision was made as usual along the border of the biceps, the fascia of the arm being laid bare at the first stroke of the knife, and opended by the second. On introducing the finger into the wound, I felt the pulsation of two arteries—the one in the usual position of the brachial, close beneath the border of the biceps—the other toward the ulnar border of the arm, but not remote from it. On

compressing the latter, the pulsations of the tumor did not cease, but when this was practised upon the former, they were instantly commanded. The pulsations of these vessels were so equally strong that I immediately inferred that, in this instance, the division of the brachial had occurred at a higher point than usual, and that only the radial branch was concerned in the operation. With this impression I immediately applied the ligature to the artery beneath the biceps. The pulsations of the tumor at once ceased, its volume diminished, and it became flaccid. A silk ligature was employed, one extremity of which was left hanging from the wound. The incision was very accurately closed by means of adhesive strips—a compress was applied to the tumor, and a roller to the member, from the hand to the shoulder. The patient was kept in a tranquil state. At the same hour of the next day the tumor was found, on examination, to pulsate feebly. There was also a pretty strong pulsation in the radial artery, at the wrist. The brachial artery was also found to pulsate slightly where it merged itself in the tumor. I could not now feel the pulsation of an artery toward the ulnar border of the arm, which could be regarded as the ulnar artery, nor were the pulsations of the ulnar at the wrist stronger than those of the radial, and I now came to the conclusion that the collateral vessel, which I so distinctly felt during the operation, was the anastomotic, considerably enlarged in consequence of the circulation in the brachial. There had occurred no visible diminution of temperature in the limb. The bandage was re-applied and kept wet with cold water.

On the third day the bandage was again removed, and we were gratified to discover that there now no longer existed any pulsation in the tumor, although it was still manifest in the artery both above and below it.

On the fifth day there was still no pulsation in the tumor, and it had now become quite hard and incompressible. There was not much tenderness to the touch, nor had the patient experienced much pain, though it was occasionally such as to disturb his rest at night.

On the thirteenth day the ligature came away. In the mean time complete reunion of the wound by adhesion had taken place, and he had scarcely felt a sensation where the ligature had been applied, till the present time. By any prudent effort we could not now, by pressure, force any fluid out of the cyst into the artery, or in any measure diminish its size. It was manifest, therefore, that the blood contained in the cyst had now formed a firm coagulum, and that we might confidently expect its ultimate obliteration.

On the twentieth day the patient complained of severe pain and tenderness in the tumor, and evidently a slight degree of inflammatory swelling had taken place at its base. I conjectured that suppuration of the cyst might be about to happen, in consequence of the coagulum being so thickly covered with integuments. I directed a poultice to be applied to the part, and this to be compressed with the bandange. In two or three days the inflammation was wholly dissipated, and I found on comparing the tumor with a cast which had been made of the part before the operation, that it had lost half its original size. It was perfectly free from pulsation."

III. *On the mode in which Colchicum operates in the cure of Acute Rheumatism.*—We are indebted to our persevering hebdominal brother, the Boston Medical and Surgical Journal, for a short paper from the Lancet, by Dr. A. T. Thompson, whom we recollect as an editor of the London Pharmacopœia, and who is a man of sound sense. The views presented by Dr. T. are so exactly in accordance with our own, that we are induced to present the article entire. It is as follows:

"The number of cases of acute rheumatism which has been admitted within the last twelve days, has again turned our attention to this disease: but I shall now direct your notice merely to one of these cases, not because the symptoms have displayed any peculiarity, but because the rapidity of the relief which followed the operation of the colchicum upon the alimentary canal; affords me an opportunity of explaining my opinions of the manner in which this valuable remedy usually produces beneficial effects in rheumatic inflammation. It has been customary to refer these effects of colchicum to its sedative or narcotic powers; an opinion which my experience prevents me from adopting; but in dissenting from this view of the influence of colchicum, I am anxious that I shall not be misunderstood as attempting to deny that it possesses sedative powers; although I have never seen these adequate to the production of permanent relief from pain in acute rheumatism. If a full dose of the wine of the seeds of the colchicum be administered after a moderate bleeding, it rarely fails to purge, and to maintain the reduction of the pulse caused by the abstraction of the blood. Under such circumstances it would be remarkable if there was no abatement of pain, if the system be brought under the influence of even a very moderate sedative. The purgative influence of the colchicum carries its sedative power, although weak, thus far, whilst it precludes the further employment of the lancet; and thus it seems to combine the advantages of purging, and the administration of narcotics. Now in taking this view of the influence of colchicum, I am convinced that it is never productive of much benefit in acute rheumatism, unless it purge freely. The evacuations are generally copious, liquid, and highly bilious, owing to the remedy, as a topical stimulant on the duodenum, exciting powerfully the orifices of the biliary duct, and causing a large flow of bile to the intestine, in the same manner as exciting the orifices of the salivary ducts in the mouth not only empties those glands, but augments their secreting function. The purgative effects of colchicum are sometimes excessive, and the consequent debility is great; but I have seen twelve or fourteen copious watery stools produced by a full dose of the drug, without any obvious debility resulting. If the lancet be not previously employed, the excitement present in the habit seems to resist both the purgative and sedative powers of colchicum; which, apparently, like those of some other remedies, require the habit to be brought into a certain condition, before they can operate in a salutary manner. It is on this account that I almost invariably, as you must have ob-

served, order my patients with acute rheumatism to be bled, and the bleeding to be followed by a dose of calomel, tartar emetic, and opium, before prescribing colchicum. If the patient be of a plethoric habit, f. ʒj· or f. ʒiss. of the wine of the seeds may be given for a dose, six or eight hours after the administration of the pill; and this dose is to be repeated once in six hours until it begin to purge, when either the repetition should be deferred for twelve hours, or, if the effect be powerful, the medicine should be altogether discontinued. If febrile symptoms recur, accompanied by pain, I have generally found the tartar emetic preferable to a return to full doses of the colchicum; but the influence of the antimonial is aided by small and frequently repeated doses of the colchicum. On the contrary, if the pulse remains soft, regular, and moderate in frequency; and the pains do not return, except perhaps in a slight degree in the evening; whilst the skin, also, remains cool, I have found that the decoction of yellow bark, acidulated with sulphuric acid, or the solution of the sulphate of quinia, acidulated in the same manner, tends more than any other means to secure the patient from a relapse, and to confirm the cure.

In making these remarks, it would be uncandid were I not to mention that most distressing effects have ocasionally followed the employment of large doses of colchicum; but these have either depended on the improper administration of the medicine, or on idiosyncracy. As a rule to guide you against the first error, you must recollect that colchicum ought never to be prescribed when the red or glazed state of the tongue indicates much irritation or a sub-acute inflammatory condition of the mucous membrane of the alimentary canal. In this case, moderate doses of the hydrocyanic acid and liquor potassæ with Dover's powder, aided by counter-irritants, may be employed to prepare the habit for the use of the colchicum; and even after the irritable state of the alimentary canal has been sufficiently subdued to admit of its use, the remedy ought to be prescribed in small doses, frequently repeated, instead of the full doses that may be administered under other circumstances. The deleterious influence of idiosyncracy can only be avoided by inquiring into the effects of the remedy when it has been taken at any prior time."

We have lately treated two cases of sub-acute rheumatism with the vinum colchici and laudanum with decided success. One of them, only, was under our immediate observation. Originally attended with intense inflammation, travelling from joint to joint, and resisting most copious depletion by the lancet, tartar emetic, calomel, etc. we still found it, after the lapse of many weeks, an obstinate and painful affection. Under these circumstances, we gave the wine of colchicum in drachm and two drachm doses, in the evening with laudanum. It invariably purged the patient, and, both before and after that effect, afforded him signal relief—indeed, soon worked out a cure.

It is worthy of remark, that in no stage of this disease was there any tenderness of the spine.

IV. *Practical and pharmaceutic remarks on the Veratrum Viride.*—In No. 32 of the Amer. Journal, the profession are favored with a paper by Dr. Osgood, of Rhode Island, on the Veratrum Viride, or "American hellebore, swamp hellebore, Indian poke, Indian uncas, poke-weed, bear-weed, itch-weed, tickle-weed." We have copied out the whole of the catalogue of popular synonims, that our non-botanical brethren (unfortunately by far too numerous) may perchance be directed, by some one of them, to a knowledge of the plant itself.

The *Veratrum Viride* appears to be an active medicine. According to professor Tully, as quoted by our author, it is, 1st. deobstruent or alterative, (rather indefinite for the 19th century;) 2d. acrid narcotic; 3d. emetic; 4th. epispastic; 5th. errhine. Passing over a commentary by Dr. Osgood on each of these properties, we shall present our readers with an account of some experiments made by him and one of his friends, on themselves.

"At 12 o'clock, M. I took two grains of the finely pulverized extract. At 1 began to experience a slight sense of uneasiness at the stomach, but not amounting to nausea. This uneasiness at the stomach, though so slight as to be attended with very little inconvenience, continued till about half past 1, when vomiting commenced. The contents of the stomach were thrown off without nausea, but with a sense of rising in the œsophagus, which perhaps might be compared to the rumination of animals. Judging from my sensations at the time, should suppose the muscular fibres of the stomach contracted gradually and steadily upon its contents, until they were expelled, the diaphragm and abdominal muscles remaining entirely inactive. After the vomiting had continued a considerable length of time, it appeared to be more the effect of spasmodic action, and was attended with chills and coldness of the whole body, but moisture of the skin. At the expiration of about an hour, vomiting ceased, and was followed by dimness of sight, dilatation of the pupils, vertigo, faintness and somnolency, pulse at the wrist 40 in the minute, and scarcely perceptible. I then took 25 M laudanum, and fell asleep. After the lapse of an hour, awoke with a continuance of the same symptoms, together with a dull pain in the epigastrium, and immediately repeated the laudanum. But finding no relief, the dimness of sight increasing, and on motion of the body, or turning the head, amounting almost to blindness, a sensation of stiffness in the voluntary muscles supervening, particularly the temporal and extensors of the head, together with considerable general prostration, the dose of laud-

anum was doubled. This produced a partial abatement of the symptoms, and after another similar interval was repeated, with half a gill of brandy, which soon effected entire relief. In connection with these symptoms, it should be observed that I am unusually susceptible to the operation both of narcotics and emetics.

The individual to whom I have alluded as also taking this extract, may perhaps be considered as at the other extreme in the range of susceptibility. He commenced at 9 o'clock in the evening, with two grains. In ten or fifteen minutes' slight uneasiness at the stomach; at half past 9 took four grains more; at 10, a sensation of something like a ball rising in the œsophagus, which seemed to extend up as far as the top of the sternum, as if propelled by a gradual tonic contraction of the stomach. At quarter past 10 vomiting commenced. This was attended with very little inconvenience at first, but after continuing a short time became more severe, the ejections consisting principally of bile; together with the vomiting, there was much ineffectual retching; almost constant hiccough; chillness; dimness of sight; vertigo; inability to control the voluntary muscles; distress at the stomach; pulse small and creeping, and 34 in a minute; the ordinary frequency ranging from 56 to 58. As these symptoms were becoming more aggravated, he took ʒss· of laudanum, and went to bed scarcely able to walk. In ten or fifteen minutes the laudanum was repeated, which soon produced sleep. In the morning was apparently in better health than he had been for several months. At 7 the same morning, three grains more were taken; at 9 complained of a confused sensation in the head, and almost an entire loss of power of the gastrocnemii muscles. At 12 M. three grains more were taken, and at half past 12, all the muscles of the forearm were affected in the same manner. At 1, vomiting; pulse 40, and other symptoms essentially the same as the day before, excepting a less degree of chilliness. At half past 2, took 45 M laudanum, and in the course of two hours the effects of the medicine entirely subsided, excepting the inability of using the gastrocnemii muscles. At 11 in the evening two grains more were taken, which, in about three quarters of an hour, produced vomiting like the other cases, but without any appreciable narcotic effect." p. 302–3.

The following observations present it, as a valuable medicine in a group of very painful and too often refractory diseases:

"It is unnecessary fully to enter upon a therapeutic application of this article, or to enumerate all the diseases in which it has been employed. Among those in which it stands foremost in the list of remedial agents, are the arthritic inflammations. In this class of diseases, it should be given in such doses as at first fall short of producing disturbance of the stomach, as one-third of a grain of the extract, or ʒss. of the tincture, regularly repeated every three or four hours, and gradually increasing to the extent of producing narcosis or vomiting on the one hand, or resolution of the disease on the other. To insure its best effects, opium in moderate quantities should be used. A combination of the wine, with the tincture of opium, in the proportion of three parts

of the former to one of the latter, cannot, it is said, be distinguished in its operation from the celebrated *eau medicinale*, excepting by the catharsis which sometimes ensues from the use of the latter. This combination is much more efficient than the wine or tincture alone, producing less disturbance of the stomach, and can be employed in larger quantities without inconvenience from its narcotic effects. In *gout*, of the regular kind, this article manifests its best powers. It is the opinion of Dr. Tully, that with proper management it will cure a majority of cases. It proves most successful in those constitutions which are not impaired by habits of gluttony and intemperance, at the same time it is much better adapted to broken down constitutions, than the colchicum or veratrum album, on account of the exhaustions these articles are liable to produce. If used in efficient doses, and perseveringly continued several days, there are few cases but will be decidedly benefitted, if not radically cured. It appears to be as well adapted to rheumatism as gout. In the treatment of that disease, both in its acute and chronic form, the article is well worthy the attention of the profession. There is no remedy in the materia medica within my knowledge, with the exception, perhaps, of the actæa racemosa, to which *acute rheumatism* more easily yields. In this disease it should also be combined with opium, for the purpose of relieving pain, and qualifying its effect upon the stomach. The amount of opium conjoined, should be graduated in some measure by the severity of the pain. Thus qualified, it should be administered at regular and short intervals, generally as often as every three hours, in such doses as at first fall short of producing nausea, and gradually increased. Thus administered, the system is kept under its steady and uniform influence. If the doses fall short of producing its specific effects upon the stomach and brain, or if administered so irregularly that the effects of one dose pass off before another is given, but little will be accomplished. In a common case of acute rheumatism, a cathartic of calomel should first be premised, unless the bowels are in a relaxed condition, or some other circumstance exists to contravene this practice. If the stomach is not in an irritable state, it is then best to commence with f. ʒj. of the tincture, in the combination recommended in gout, each dose to be increased 5 or 10 M, as the case may require, till some effect is produced. All local applications should be avoided, as in no way promoting the operation of internal means, and only liable to draw the disease from one part to another, where perhaps its presence is still more to be dreaded. The more acute the disease, the more erratic in its character, and the earlier in its progress, the more speedily does it yield to this course of medication. In *chronic rheumatism*, unattended with inflammation and swelling of the joints, it is less successful than in the acute, from its being a less controllable form of the disease. In this variety, however, it is probably more efficient than any other remedy of equal safety which we possess. It is often necessary to continue its use several days before much benefit is perceived." p. 305–6.

We shall conclude this notice with a paragraph on the pharmacy of our medicine.

"The pharmaceutic preparations of this plant are tincture, wine, extract, ointment, infusion, decoction, and powder of the root. Of

these, the tincture, wine, extract, and ointment, are the most eligible forms, and for common medicinal purposes appear to be all that are requisite.

The tincture is prepared by adding the recent bruised root, ℥vj. to diluted alcohol, Oj. I was formerly in the habit of using ℥viij. to the pint, but this appears to be more than is necessary for saturation; medium dose from f. ℥ss. to f. ℥j. For the wine, recent bruised root, ℥vj.; white wine, ℥xiv.; officinal alcohol, ℥ij. The alcohol is necessary to prevent the preparation from becoming sour in warm weather; dose the same as of the tincture. In reference to the relative value of these two preparations, I do not know that any thing can be said in favor of the wine, which would not with equal truth apply to the tincture. In medicinal efficacy, there appears to be no appreciable difference. The extract is made simply by expressing the juice of the recent root, and inspissating in the sun. Thus prepared, it is hard and dry, of a grayish color, and capable of being reduced to an impalpable powder. It requires a considerable quantity of the root to produce much of the extract. To obtain the juice it must be strongly bruised and subjected to strong pressure." p. 304.

V. *Case of Extirpation of the Thyroid Gland.* By N. R. Smith, M. D., Professor of Surgery in the University of Maryland. (N. A. Archives, for Aug. 1835.)

Professor Smith tells us that "the origin of the disease was as far back as 1815, beginning *at no time.*" What was its duration? Patient about 40 years of age, the mother of several children, sickly aspect—had lived many years "in a malarious" district, and had a *severe bilious fever one year before the operation.* The tumified gland extended from the os hyoides downwards, so as to hang over the margin of the sternum. A drawing which accompanies the case is said to afford a correct "idea of its size and form"—to our apprehension the tumor is represented as lying with its lower portion two or three inches on the sternum.

"The general configuration of the tumor, in short, was that of common goitre, and *when* the integuments were entire, it presented the varicose veins usually conspicuous in that disease."

We suppose the above sentence is intended to convey the opinion, that this was a case of goitre. But Professor S. did not see the varicose veins, he saw the gland greatly enlarged and ulcerated; and it could not be healed. There was now

issuing from the breast a serous fetid discharge, and a fungus having "*a strong resemblance to fungus hæmatodes.*"

Professor Geddings joined in the opinion that this was an enlarged thyroid gland, the right lobe being chiefly concerned. The superior arteries could, as is usual, be felt passing down to the gland. The gentlemen satisfied themselves that "the great vessels of the neck were not involved in, or adherent to the tumor." (It was found afterwards that they were adherent to the tumor.) That the attachments were slight, was, we suppose, to be expected, owing to the tumor's having long been dragging down towards the sternum.

It was determined, in consultation, that the only resource that afforded any prospect, was the entire removal of the disease by extirpation. But, we might ask, who has the tact for removing *entirely an ulcerated malignant tumor*, which this certainly was?

Some time after 1815, or on — of ——, Professor Smith performed the operation. "An elliptical incision, the length of which corresponded with the sack," was made. The dissection was now commenced on the right side, and so directed as to aim at tying the superior thyroid artery on that side: it became necessary to tie it more than once, its tortuous course bringing it under the knife.

In turning back the sterno-mastoid muscle, which lay hard upon the tumor, the anterior jugular vein was cut. The vein being emptied of its blood, by unavoidable pressure, the operator became apprehensive of great risk, from the entrance of air into the wounded vein, and strove to avoid the accident, by compressing the vessel while tying it. The utmost care did not serve to prevent the admission of a small portion of air into the vein, and excited great anxiety in the mind of Prof. S., but no visible evil arose from it, though three portions were admitted during the operation.

It will be recollected that a fatal accident of this kind befel Dupuytren, in operating upon a tumor of the neck. This case is sufficient to convince us that such accidents may occur; they are, however, fortunately very rare; and Prof. S, having seen this occurrence three times previously, and three

times in the present operation is strikingly singular. We have seen large veins again and again laid open, in attempts at suicide—in one instance the external jugular was divided, and the patient had nearly bled to death, no air was drawn in or suffered to enter. We have often removed large tumors of the neck, but have not seen any thing of the occurrence in view. Our memory does not furnish us with more than one fatal accident of this kind, and how many hundreds of tumors have been removed? how many wounds inflicted upon the neck? Besides, Professor Smith's own experience, on this point, shows that danger can only arise from carelessness, since he witnessed the entrance of air on four different occasions, and no evil followed—why then his great fears from the ingress of air into veins? We would not, however, wish to divert attention from this source of danger, be it ever so slight; nor to excite fearful apprehensions which may tend to embarrass operators, when it is sufficient that they keep the risk in view, and calmly avert the possible danger, by compression on the cut ends, but by no means tying all the veins.

We are reminded here of another risk from wounds of the veins of the neck. We were once called to a man, who, in a desperate attempt to commit suicide, drew a razor, with a rounded cutting end, across the throat, so as to make a considerable wound, and then thrust the end of the razor down into the left side of the neck, till it reached the cervical vessels, and, turning it round, he uncovered the carotid artery, a little below its bifurcation. Great hemorrhage took place, and he lost several quarts of blood, it was supposed, before we arrived. The parts being much deranged, and injected with blood, we found it necessary deliberately to dissect, so as to lay bare the great cervical vessels, and now we found that they were not cut, and that all this vast and dangerous hemorrhage came from the external jugular vein. We applied an animal ligature, and the hemorrhage was arrested at once. We saw this case in consultation with our friend, Dr. S. B. Martin. Now although hemorrhage may be said to be slow from veins, compared to that of arteries; yet, it is sometimes

such as to lead to danger, and should not be lost sight of, particularly when the surgeon cannot remain near his patient.

The dissection having been carried through the more superficial vessels, it now became more easy, particularly on the left side. On this side was heard the ingress of air again, and induced the operator to hold each portion of structures between the thumb and fingers, while they were divided—the lower portions of veins were secured by ligatures. Can it be necessary, after so many successful operations, upon the neck, to tie all the veins? Is there not considerable risk of phlebetis? I should have no hesitation in trusting to a careful pressure of the ends of cut veins, for a minute or two, between the thumb and forefinger, unless there be some thickening of the venous coats, which may cause the vessel to remain open; and, I strongly suspect, that some degree of density of the coats must exist, to give rise to a dangerous gurgation of air. If we do tie, the animal ligature should be used.

"The sheath of the great vessels on each side lay in immediate contact with the tumor, and, indeed, was to some extent incorporated with it. I also found that the fascia covering the thyroid and cricoid cartilages, was thickened, and adherent to the tumor. Its separation from these connections was the most difficult and tedious part of the operation. The separation of the tumor from the parts immediately above the border of the sternum, where I was prepared to expect most difficulty and danger, was effected with infinitely more ease than I had anticipated." The narrative goes on to state, that the vessels were elongated, contracted in their volume, and pulsating feebly; many veins obliterated; the right inferior thyroid artery cord-like, the left missing. A few branches of vessels were secured, in the lower part of the wound; the trachea was nearly laid bare down to the sternum.

It should be remembered that any anomaly in the large arteries, at this point, might be a source of danger—the high bifurcation, which we sometimes see in the arteria-innominata, might create considerable difficulty and risk.

Looking upon the wound, the great vessels of the neck were seen beating—the recurrent laryngeal nerve was seen

on the right—the trachea bare. Where was the œsophagus? The wound was closed with the interrupted suture, and dressed, leaving the patient pretty comfortable during the first day. The next morning considerable hemorrhage took place; the wound was opened, and a ligature applied to a small artery, lint and alum to less important vessels. The hemorrhage seems to have prostrated the patient considerably, and, together with the distress of re-dressing, produced great prostration. "She did not swallow without effort, but without difficulty."

The patient was supposed to be doing well, until the fifth day, when a severe rigor, followed by fever, took place. *The breathing was sonorous and embarrassed*, cough and irritability of stomach existed. Are these principal symptoms usual concomitants of bilious fever? But the paroxysm passed off, with "a sweating stage, *precisely as a paroxysm of fever*. The wound, which had suppurated kindly, now began to exhibit a flabby appearance, and to discharge an unhealthy secretion."

"The chills were evidently of malarious origin, and the patient undoubtedly came to me predisposed to intermittent fever. To this, in part, I think I may with propriety ascribe the fatal result."

It would perhaps be thought invidious to deprive Professor Smith of the pleasure of getting out at this loop-hole. But we must be allowed to express our doubts, whether a patient greatly prostrated, by ulcerated fungus tumor, of long duration, was not more likely to die of the effects of the operation, than from intermittent fever, simply because she had lived in "a malarious district," although she had had no disease ascribable to malaria for a whole year prior to the operation. Why is not the period of the year made to fill up two blanks, in the report of the case? If such an operation would not make a patient undergo sweating stages and fever, and even death, one cannot well conceive what could so affect a human body.

We are at all times willing to let practitioners make their own decisions, and, occasionally, boldly step forward to haz-

ard a difficult and dangerous operation, where there is any reasonable hope of saving a patient, but let them always call things by their right names. We, looking on the result, say, that in such a habit, with a disease so protracted, and so evidently malignant, and beyond the proper period for operation, the patient ought not to have been subjected to so formidable an operation: apart from this important consideration, Prof. Smith has performed a difficult, and, speaking of the execution merely, splendid operation; but, we think, if he had left the patient to the intermittent fever single-handed, it would have been better for her, and just as well for surgery. We admonish our younger friends, in the profession, not to grapple with ulcerated malignant tumors of the thyroid glands, otherwise they may lose all such patients, even without the consolation of *hoping* they may have died, "in part," of intermittent fever.

Professor Smith would offer this case in support of the opinion that the dangers of extirpating the thyroid gland have been overrated. But does not the narrative of the case show that the vessels of greatest difficulty, nay, the only ones of difficulty, were not in their normal condition; on the contrary the right thyroid artery pulsated feebly, was hard and elongated; the left, of all things the most difficult to manage in the removal of the thyroid gland, was wanting — shall we, nevertheless, admit this as a good proof of the safety, and unexpected facilities attending the performance of the operation? What does this operation prove? That an enlarged fungating thyroid gland may be removed; all the vessels laid bare as well as the nerves; vein after vein tied; the trachea denuded throughout its whole length; the œsophagus exposed; and, still the patient may bear the operation; but it does not prove that recovery may or can take place. If we admit that it was meritorious to hazard much to rescue a deserving woman from death, the result shows that under similar circumstances it should never be repeated.

Our author seems desirous of having it understood that he extirpated the entire gland. If he has given an accurate description of the operation, which we admit, there can be no

doubt on this point; but did he take away all the contaminated parts? The termination shows what we may know, *a priori*, that it was not, nay, could not be done.

As to the dangers of the extirpation of this gland, we know that among the earlier successful attempts, may be mentioned those of Desault, who tied the thyroid arteries, as the first step of the operation; and Mr. Gooch, who saved his patient by making compression, by the aid of a succession of fingers, for eight days.

What shall we say to the declaration that "the above case appears to me to warrant the belief, that it may be undertaken with hope of success"? Now, we know that this operation has often been successfully performed, under proper circumstances. Does it follow, that it will succeed when performed under improper circumstances? Read the history of ulcerated tumors, "resembling fungus hæmatodes," and look at the result of the case under review, and then tell us, who is the man to repeat this operation, under like circumstances; or say, that, inasmuch as intermittent fever may have come to aid the business of death in this case, therefore, it may be held up as worthy of a place among our precedents in surgery. Our calmer reflections will not allow it such a place. H. G. J.

VI. *Researches on the Symptoms and Diagnosis of Aneurism of the Thoracic Aorta.* By George Green, M. D., Fellow of the Royal College of Physicians, etc., etc.—We are reminded, in this interesting paper, of the difficulty which has existed in detecting aortic thoracic aneurism, in its incipiency —that Laennec was not aware of the utility of the stethescope, in this respect—that without the aid of this instrument this form of aneurism cannot always be distinguished from other tumors—that Bertin and Dr. Hope maintain, that aneurism may be detected in the chest, by means of the stethescope, as certainly as affections of the heart or lungs.

Dr. Green, entertaining views in accordance with those of Bertin and Hope, details several cases in support of the opin-

ion, that the stethescope, by careful and repeated applications, will serve to point out thoracic aneurism, before there are any other evidences of such disease.

Case 1. *Double aneurism of the thoracic aorta. Sudden death from rupture of the sac, into the left bronchial tube.*

Luke Moran, a sawyer, was admitted into the Whitworth Hospital, 7th of April, 1834. For two years he had had a cough, attended with great dyspnœa, and frothy expectoration —the cough paroxysmal, generally at night—there were irregular lancinating pains throughout the chest. Symptoms so severe as to prevent his attention to business the last six months.

At the time of admission, he coughed for half an hour, and felt a sense of impending suffocation. The least exercise increased the dyspnœa; most distress at the top of the sternum. During inspiration the trachea was deeply drawn behind that bone—'breathing laryngeal,' there were darting pains through the chest, slight dysphagia—the radial pulses the same; no œdema or numbness of the upper extremities; the chest sounded clear all over, on percussion, a distinct impulse was given to the stethescope, in the upper third; and, on either side of the sternum, stronger than that of the heart; no distinct bruit; respiration in the left lung feeble; loud and clear in the right, especially on a forcible inspiration. A distinct impulse down the back, on the left of the spine, no purring or tremor present, a distant but distinct bruit de soufflet; sounds of the heart normal. "When the instrument was applied to the axilla, or to the back, in the situation mentioned, the respiration appeared to be almost laryngeal in the top of the left lung, but was natural in the right, and loud and distinct." Five days after admission this patient died suddenly from rupture of the aneurism.

This case, having been advanced before the patient was admitted into the hospital, does not afford any thing directly in support of the point which Dr. Green proposes to establish, viz: that the stethescope may be applied to discover aneurism of the thorax, before it is cognizable by any other means. It does not, therefore, seem necessary to pursue the case fur-

ther. It may suffice to say, that, as usual in this form of aneurism, the coats of the aorta were found diseased, having "atheromatous deposits" between its coats.

Case 2, Was a woman of 27 years of age, full habit, florid complexion—admitted into the Whitworth Hospital, 8th of January, 1835. About two months before admission she fell down stairs, on her back, after which she felt pain and weakness in that part. The pain is now seated from the second to the fourth dorsal vertebræ. Six weeks before admission she was attacked by a severe cough, a sense of tightness across the chest, and oppression. The cough was loud, harsh, and of a peculiar ringing character; and there was a retraction of the trachea, deep behind the sternum, similar to that noted in case first—there was frothy expectoration somewhat copious. The cough came in violent paroxysms, chiefly at night; threatening suffocation, and obliged her sometimes to sit up; some difficulty in swallowing, which is referred to the upper third of the sternum, where the morsel appears to stop, and requires the aid of drink to convey it down. There is pain on pressure of the third and fourth spinous process of the dorsal vertebræ. The pain is of a shooting character, shooting across from the spine to the sternum, and completely traversing the thorax. Pulse in both wrists the same as to force and frequency. No numbness, or œdema, of the upper extremities; respiration hurried.

Percussion afforded a clear sound all over the chest; respiration in the right distinct, but attended by a blowing sound, when the instrument was applied posteriorly, about the situation of the bronchial tube, or in the axilla of the right arm. Respiration in the left lung was feeble and sometimes nearly inaudible. An obscure impulse, accompanied by a bruit de soufflet in the interscapular region. Both of these phenomena most distinct at the left side of the spine, and confined to the part where she felt pain from pressure, most remarkable when the arterial system was excited. Anteriorly an impulse was communicated to the stethescope at the sterno clavicular articulation, and sub-clavian region of the left side. The impulse was attended with a double sound, and was some-

what stronger than that of the heart. An obscure bruit de soufflet was heard in the same situation. The impulse and sounds of the heart were natural. She complained of pain upon pressing the top of the sternum. The symptoms were occasionally moderated by general and local bleeding.

Two days before the patient's death, and twenty-sixth after her admission, the following note was made. There is a strong double impulse a little below the sterno-clavicular junction; at the left side of the medium line of the sternum, accompanied by two distinct sounds, and a clear bruit de soufflet. Impulses diminished towards the heart, which retains its natural impulse and sounds. No expansive pulsation on pressing the fingers deep behind the clavicles; left jugular vein turgid. The stethescope detected a bruit de rape, this diminishes as the instrument is carried down on the left side of the spine. There was now to be seen an elevation by looking along the anterior surface of the chest, at the top of the sternum. Here, too, pulsation can be felt, and percussion produces pain. The dyspnœa caused great alarm. Examination after death disclosed the rupture of an aneurismal tumor of the aorta. Another aneurismal tumor was found in a state of moderate development.

We have reason to believe that in this case the stethescope aided, in some degree, to ascertain the nature of the disease in its early stages; but the case is not calculated to be useful, so much on this account, as in regard to its serving to awaken closer attention to the stethescope, as a means of discovering aneurism, when concealed in this deep and hidden situation.

Case 3. This case having been detected whilst the aneurism must, as Dr. Green supposes, have been in a very incipient state, is on that account important. Michael Hughes, a butler, aged 38, a stout, well made man, was admitted into the Meath Hospital, complaining of great dyspnœa, cough, and wandering pains about the back and chest. Chest sounds clear throughout on percussion. Respiration in the right lung intensely peurile, altogether absent in the superior portion of the left, and only audible in the interior portion. The patient drawing in a deep breath while the stethe-

scope is applied to the left axillary region, no sound is heard during the first half of the inspiration, but, during the latter half, the air appears to overcome some obstruction to its entrance, and suddenly rushes in to expand the lungs. The left side of the chest is almost immovable during respiration, but the right acts freely. On applying the hand to the upper part of the left side, no vibration of the voice could be detected. The vibration is very feeble on the same side, but well marked on the right side; impulse and sound of the heart natural; a strong double pulsation is heard over the sub-clavicular region of the side, also over the postero-superior portion of the same side, and on applying the hand over the former situation distinct impulse is communicated to it. No bruit de soufflet could be detected. The cough is hard and ringing, and of a peculiar croupy character; expectoration scanty, consisting of frothy mucus; respiration hurried. Pulse 86, and moderately full; tongue clean, appetite good; never had any difficulty in swallowing, or pain in the throat. He attributes his complaint to cold caught five months ago from sleeping in a damp pantry, shortly after which he became affected with perspirations, wandering pains in the back and sides, increased upon exertion, and attended with some dyspnœa. The pain in the back he describes as being of a lancing nature, resembling that which would be inflicted by a knife. The difficulty of breathing increased, with inability to use any violent exertion, and after some time was attended by a loud dry cough.

This patient was afterwards examined at the Richmond Hospital, by Dr. McDowell, at which time there was a striking sameness of symptoms—a gradual increase of severity in the symptoms took place till the patient died suddenly during a fit of coughing, throwing up a good deal of blood just before death.

Dissection disclosed the bursting of an aneurismal tumor within the thorax; and, as usual, the arterial coats were found diseased. Up to the death of this patient the chest sounded clear on percussion, showing, of course, that no aid is to be derived from this expedient in cases of aneurism.

Our examination of the foregoing cases leads us to the conclusion that we cannot abridge Dr. Green's reflections upon these, without lessening their importance. However little we may gain, in a practical point of view, by the more intimate knowledge of the signs of thoracic aneurism in its incipiency, still it is desirable that we be able to detect this disease early. A proper prognosis may enable us to advise paliative measures, at least, and often with the consolation that our knowledge serves to fix our attention, and enables us to advise on that correct knowledge. Moreover, nature having now and then conducted these affections to a favorable termination, leaves a slight hope that, by a well directed course of conduct on the part of patients, and correct advice of the physician, they may be treated with hope of prolonging life, and a belief of possible restoration.

A strict avoidance of all exciting causes, whether it be in regard to exercise, food, mental quietude, occasional depletion, etc., may sometimes, no doubt, be of great advantage to persons thus unfortunately situated. Nor should the plan of abstinence, and other depletory means, be lost sight of, even with the hope of curing in the early stage of the disease—we mean the plan, or something similar to it, of Valsalva.

H. G. J.

The result then of these cases, together with those already published, particularly by Dr. Stokes* and Bertin,† proves that we are in possession of a number of physical signs, which, when attentively studied in connection with the general symptoms, will lead us to a knowledge of this obscure disease, even before it has made any considerable progress. The necessity of taking every phenomenon into consideration, is obvious. We should recollect that our diagnosis ought never to be founded on one or two, but on a series of observations, carefully noted and compared with each other.

I have before mentioned that there is a portion of the aorta where some of those signs must necessarily be absent. As far as the different portions of the arch of the aorta are concerned, the diagnosis of aneurism of the vessel may be said to be in a great measure rescued from the obscurity in which it was formerly involved, and for which we are much indebted, amongst others, to the observations of Dr. Stokes.—But when we come to consider the signs and symptoms we are able to collect when it occurs in the remainder of its course in the thorax, we

*Dublin Medical and Surgical Journal, vol. v.
†Observations 37 and 38, Maladies de Cœur.

find that they are diminished in number, and are often so feebly characterized, as to escape our notice. It may be superfluous, perhaps, to give an example in proof of this assertion, when so many are already on record. The following case, however, which occurred at the Whitworth Hospital, is so much in point that I will here mention it. The case was taken by Mr. Mayne, who was clinical clerk at the time the man entered the hospital. I had not myself an opportunity of examining the patient. I shall give a very condensed account of it from the notes furnished to me by Mr. Mayne.

Laurence Duffy, aged 43, admitted 12th July, 1833, states he had been healthy all his life, until about nine months previous to his admission, when he began to experience a difficulty in swallowing, the increase of which induced him to apply for assistance. When questioned, he complained of difficulty in swallowing, which he referred to the centre of the sternum, where the effort was attended with pain followed by vomiting. Fluids passed down without difficulty; his bowels were constipated; the chest sounded well throughout; the respiration over its entire extent was perfectly natural. His bowels were freed by a calomel bolus, and purgative mixture. A slight cough, which he had on his entrance, altogether disappeared for seven days after his admission, and no symptom whatever was complained of, but the dysphagia. He died suddenly on the night of the seventh day from his admission.

On dissection, an aneurism was found corresponding to the fifth and sixth dorsal vertebræ, about the size of an orange, and it had burst into the right pleura. The tumor had slightly bent the œsophagus, which formed a curve, the concavity towards the sac.

Here then is an instance where, if we were to rely on the general signs, we should have had but one to excite our suspicions of the disease; and this should lead us in all cases of long-continued dysphagia, to institute an accurate examination for the physical signs of aneurism. A question then naturally arises: what are we to expect from such an examination, when aneurism exists in this situation? It will be obvious that when the mouth of the sac is situated below the fourth dorsal vertebræ, we cannot expect to find the same modifications of the respiration as those above mentioned. I mean this remark to apply to the incipient stage of the disease, for when it is considerably advanced the expansion of the sac upwards may of course produce pressure on either of the bronchial tubes, but even such an occurrence could not happen for a considerable length of time, when the tumor is situated opposite the eigthh or tenth dorsal vertebræ. It might perhaps be important to observe in such a case, whether the patient complained of pain on deep inspiration at the postero-inferior portion of the lung, or whether pains resembling those of pleurisy exist about this situation, for if the sac should contract adhesions to the pleura, it might not be unreasonable to expect pains similar to those that precede the advance of tubercles to the surface of the lung. When the sac has so far increased as to produce an absence of respiration in any considerable portion of the lung, the nature of the case will be, in all probability, apparent from other symptoms.

On reflecting upon the symptoms which would attend an aneurism so low down in the thorax, I at first imagined, that if the tumor produced an impediment to the aortal circulation, we might be able to ascertain it by the feeble impulse of the abdominal aorta, when the steth-

escope was pressed firmly down upon it, some way above the umbilicus, but on considering the cases that have been published of obliteration or contraction of this vessel, I am not inclined to suppose that this phenomenon would exist. Many cases of such a change in the artery, as I have mentioned, are now on record. The latest published is in an interesting communication from Mr. Nixon, in the fifth volume of the Dublin Medical Journal, where copious references are made to those previously noticed. From examining the plates which illustrate these contractions of the vessel, it will be seen that the aorta immediately below the constriction is abundantly supplied by collateral vessels, and no perceptible diminution of the calibre of the tube can be recognized. It is remarkable that this phenomenon has occurred in so many cases, near the situation of the ductus arteriosus, and it may become a question for further consideration, whether the process established by nature for the closure of that tube at birth, may not be extended to the aorta in its vicinity, and thus constitute either a congenital malformation, or establish a predisposition to aneurism in this part of the vessel.

The collateral circulation, I have alluded to, is attended with such an enlargement of the vessels that carry it on, that I was in hopes we might be led to infer the existence of the stricture, by attending to the increased pulsations in these arteries; but, unfortunately, they are so numerous and deeply seated, as to be without the reach of any means we can apply to discover their pulsations. In all probability, also, should the closure occur from the pressure of an aneurism, these changes in the collateral vessels will be a gradual process, so that no sudden diminution of the pulsation of the vessel could be discovered below the stricture. The principal alterations that occur in these cases, are in the intercostal arteries, and their anastomosis with the mammary, etc. as is well seen in a plate in Meckel's Archives, 1827. The case of constriction, published by Meckel, occurred about the situation of the ductus arteriosus. It was accidentally discovered in the body of a patient, who died at Bonn, but of whose previous history nothing could be ascertained. In the plate which illustrates this lesion, and for the inspection of which I am indebted to Dr. Graves, who directed my attention to the case, these arteries are seen remarkably developed and tortuous, but as I have just observed, they would be too deep from the surface to enable us to recognize their pulsations during life.

In concluding these remarks, I would wish to direct attention to the obscurity which still attends the diagnosis of aneurism of the inferior descending portion of the aorta, an obscurity which does not attach in so great a degree to aneurisms of the different portions of the arch. Since I have entered on the investigation of this subject, I have not been able to examine a case of the disease in this situation, but it is not improbable that those who have, will be enabled by their observations to elucidate this interesting point of pathology.

As far as I am at present enabled to offer an opinion, the principal symptoms and physical signs on which I would be inclined to diagnose the existence of an aneurism in the situation I have just mentioned, would be as follows:

1st. An impulse limited in extent, and decreasing in intensity as the stethescope is applied above or below the situation of the suspected tumor. This sign is more valuable, if it occurs on the right side of the

spinal column, which is not the situation, anatomically speaking, where an impulse should be perceived.

2d. A bruit de soufflet or a bruit de rape, heard in the suspected tumor, and not observable in any other considerable portion of the aorta or in the heart. It is necessary in every instance to examine the heart, for within these few days I have met with a case in the Whitworth Hospital, where a loud bruit de rape accompanies the second sound of the heart, this bruit is heard down the spine, along the course of the aorta. It is heard also in the carotid and axillary arteries, even to the elbow-joint. I have satisfied myself that in this case a double sound is heard in these arteries. The name of the individual in whom these phenomena occur, is William Connor, and several of the class of the Whitworth Hospital have heard both the bruit and the sound in the situations mentioned.

3d. The production of pain on pressing the vertebral column over the site of the impulse.

4th. The production of pain referred to a portion of the lung near the situation of the impulse, on a forcible inspiration.

5th. Dysphagia referred to some point below the middle third of the sternum, or to a point nearly opposite the impulse.

Lastly, it may be worth while to contrast the pulsations of the abdominal aorta in the epigastrium, with other arteries and with the impulse of the heart.

We should take into consideration, also, (provided the above signs are well marked,) the evidence we may obtain with respect to the position of the tumor from the absence of some symptoms, such, for instance, as the non-existence of a laryngeal croupy cough; difference of pulsation in the radial arteries; or numbness or œdema of the upper extremities.

I regret that the time afforded for getting this paper ready for the present number of the Journal, is not sufficient to enable me to consider this subject more at large. As, however, I have had no opportunity of studying the phenomena attendant on an aneurism in the part of the thoracic aorta, to which I wish to draw attention, I feel it more prudent to reserve any further observations for a future opportunity. I am sensible of having left much concerning this interesting subject as yet untouched, but in giving the cases above detailed, I shall be satisfied if they tend to strengthen our confidence in the diagnosis we are able to arrive at, by the additional aid of auscultation. The publication of such cases is the more necessary, from considering the doubtful terms in which Laennec expressed himself concerning the value of the stethescope, as applied to the diagnosis of the disease in question. We should, however, recollect, that the obscurity which the brilliancy of one mind is unable to dissipate, may be cleared up by the labors of many, and that the contributions of individuals for this purpose are, in the language of Abernethy, "like lights shining from many quarters." pp. 413–417.

VII.—*Abstract of the Evidence of Members of the Royal College of Surgeons in London, taken before the Parliamentary Medical Committee, in* 1834.

We presume that many of our readers have for some time past seen occasional notices of the piracy committed by Sir Everard Home on the reputation and works of the celebrated John Hunter. It may not be improper to state here, that Sir Everard was the brother-in-law of Mr. Hunter, and as such was appointed, in 1793, in connection with Dr. Matthew Baillie, executor of the last will and testament of Mr. Hunter. It was in this way that his manuscripts, physiological and pathological, came into the possession of Sir Everard Home. And it was from these stupendous MS. volumes, comprising the labors of Mr. Hunter's life, that Sir Everard compiled those papers which were published in the Philosophical Transactions. They did indeed promise their *supposed author* an imperishable fame; but he has scarcely been numbered with the dead, till the irrefutable evidences of his guilt have been promulgated to the world. We subjoin the evidence of Mr. Clift, extracted from the London Lancet of July, 1835, for which we are indebted to the United States Medical and Surgical Journal. It is as follows:

EVIDENCE OF MR. W. CLIFT.

211. "What situation do you hold at the College of Surgeons?"

"I have been Conservator of the Museum since the year 1800, and was appointed by the College, the Hunterian collection being placed at that time at No. 13, Castle-street, Leicester-square, which communicated with the house that Mr. Hunter lived in, and which was the house in which his collection was originally deposited. The collection was removed thence to Lincoln's-inn-fields, where at first it was deposited in the dwelling house. When half the museum room was built, the collection was conveyed into that half, and there deposited until the remainder should be completed, which was done so far as to be open to visitors in 1813. The trustees of the museum recommended to Parliament in 1806, that 15,000*l.* should be granted towards building the college, on the council binding themselves to complete the buildings *within three years*, without any ulterior expense to Parliament, but when those three years had expired, the building yet *remaining incomplete*, Parliament, in 1810, voted 12,500*l.* more, when the council again undertook to make good any further deficiency out of the funds of the college. Thus from the removal of the collection in 1806, until the completion of the building in 1813, the collection remained quite inaccessible. It was in store-rooms, in fact,—in the dwelling-house, from the top to the bot-

tom. While the collection remained in Castle-street, it was accessible to visitors, though but very few came. When I was first appointed conservator in 1800, the manuscripts of Mr. Hunter, which were descriptive of the collection in Castle-street, consisted of twenty-four fasciculi, relating to the physiological part of the collection; two volumes relating to the pathological series; and one volume relating to the fossils. The manuscripts descriptive of the pathological preparations, contained the history of very numerous cases relating to particular specimens, with such references that a good anatomist and physiologist would have been enabled to connect both preparation and manuscript; but the loss of these descriptions has greatly reduced the interest and value of the corresponding preparations; although when the government purchased the collection for the public, all the papers and manuscripts bearing any reference to that collection ought to have been given up with it, as public property; for Mr. Hunter, in his will, said, 'I also give to the said Matthew Baillie and Everard Home all my collection of natural history, and the cases, and other things belonging thereto, or used therewith, upon trust that they offer the same for sale in one entire lot to the government of Great Britain.' From the time of Mr. Hunter's death, on the 16th of October, 1793, and during nearly two years before that, (while I was Mr. Hunter's apprentice,) although not regularly appointed conservator, I had the entire charge of the collection. At the period of Mr. Hunter's death, and for some time afterwards, those manuscripts were deposited with the collection in an anteroom, and I clearly understood that they were to go along with the collection; but shortly before the collection was transferred to the College of Surgeons, those manuscripts were removed away from the collection, and I was first made aware that those manuscripts had been separated from the collection only when the collection was about to be delivered into the possession of the college by the executors of Mr. Hunter. Those manuscripts were then *taken by me in a cart to Sir Everard Home's house, by his order*, under the impression entertained by the trustess, that Sir Everard was the only person who could make a catalogue of the collection, but I had no conversation with Sir Everard at the time as to his removing them. He merely said that those papers, being a very large proportion of them loose fasciculi, were not fit for the public eye; and therefore he should take them into his keeping, for the purpose of using them in describing the collection. I did not make any remark upon that, and the trustees of the college were not made acquainted with the removal of the papers; nor was any inquiry made of me upon the subject."

212. "Would it have been possible for you, between the years 1806 and 1813, to have availed yourself of the manuscripts to compare them with the preparations?"

"It was almost impossible. The collection was put in order entirely under Sir Everard Home's direction, by me, but I had no access to the manuscripts which he had removed. Indeed, they were not necessary at that time."

213. "Why not?"

"Because the preparations were numbered, and we had merely to get them into their places according to the three folio catalogues which from 1794 down to 1806 had been prepared, two by myself, and a third by Dr. Shaw; but those three catalogues did not enter into the history of its preparations. A general arrangement of the collection on the shelves of the new museum was completed, *in a manner*, in 1813, and

even if I could have had access to the manuscripts that had been removed, I was not in a condition to make use of them until 1817, and then it was expected that Sir Everard would prepare the descriptive part of them, I being only engaged under his direction, the catalogue being supposed to be the work of Sir Evarard. He merely employed me to transcribe whatever he pleased to direct. In 1817 he began to re-arrange the collection on the shelves, but he did not supply the description of each particular preparation as he proceeded."

214. "Did it ever occur to you that in order to make a complete description, it would be necessary to have recourse to the original manuscripts?"

"Without doubt, it frequently did. In fact those reflections suggested themselves to my mind when the manuscripts were first removed, but I did not communicate with the council or any individual members of the council respecting them, until 1823."

215. "Did Sir Everard Home repeatedly promise the board of curators to draw up a descriptive catalogue?"

"He was constantly urged, *quarterly*,* by the trustees to make progress in it."

216. "The council have stated, that in 1816 it was proposed that all the curators should become joint laborers in drawing up a descriptive catalogue, when Sir Everard Home, they say, declared that it was his special duty, and that he would admit of no participation in its performance: do you know any thing of that allegation?"

"Yes, every word of it is perfectly true; I heard him declare it. The trustees had been frequently urging the board of curators to make progress in the catalogue, and as they saw that no progress was making in it, they became anxious on the subject. This was in 1817, and was the cause of Sir Everard making the new arrangement of the preparations which I have mentioned, but he only produced a synopsis,—a very slight description of the general arrangement,—which was printed in 1818, but which did not at all enter into the particular history of each case; yet, as regarded the conservatorship of the museum and the board of curators, things remained on just the same footing as before, and I continued to receive my orders from Sir Everard Home alone. By-and-by, when I was appointed conservator, and began to prepare the catalogue, I expressed a wish to obtain some of Mr. Hunter's manuscripts; and then (in 1823) the board of curators learned for the first time the extent and nature of the manuscripts belonging to the collection left by Mr. Hunter. From 1817 to 1823 I had no access to any of the manuscripts which Sir Everard Home removed, except such as he brought with him to compare with specimens in the collection, for his own special purposes."

217. "What do you mean by *his own special purposes?*"

"When he was *preparing for his lectures*, or drawing up *papers for the Philosophical Transactions.*"

217*a*. "In giving him assistance in the arrangement, did you ever ask him to let you have access to the manuscripts?"

"No;† between 1800 and 1823 I never had any conversation with him

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*Quarterly during nearly twenty years!

†Mr. Clift would seem deserving of blame for silence, or neglect, or torpidity, in this matter; yet who can doubt that had he been troublesome or inconvenient to the thief, he would have been removed from his office of assistant, and science would have been deprived of his services in rendering useful this invaluable collection? The instant that Mr.

on the subject of the manuscripts. During the whole of that period he was so intent on writing papers for the *Philosophical Transactions*, that *very little else, in the way of the collection, was thought of by him;** he *always* made his engagements in this way an excuse to the trustees for not having proceeded with the catalogue, saying that we were employed in making out the subjects which were but imperfectly understood. Though I believe that all the curators had but a very imperfect knowledge of the nature of those papers, yet *they knew that Sir Everard had in his possession all Mr. Hunter's manuscripts.* Besides which, almost all of them had been acquainted with Mr. Hunter, and *must* have known of the existence of a *large mass of manuscripts.* After the destruction of the papers, Mr. Cline stated to me, on Mr. Hunter's own authority, that Mr. Hunter had written a Treatise on Diseases of the Bones, which he intended for publication. Drawings intended to illustrate this treatise, and made by an artist who lived in his house, Mr. W. Bell, are preserved in the museum."

218. "When was it that you first received any information as to the destruction of those manuscripts?"

"I think in July, 1823. I first obtained a knowledge of the circumstance from Sir Everard himself. He began by telling me, that on that very week his house had been nearly on fire; that the engines came, and the firemen insisted upon entrance, as they saw the flames coming out of the chimney. He did not wish to admit them, but they insisted upon being admitted, and then he told me that it was in burning those manuscripts of Mr. Hunter that the fire occurred. This conversation passed in a chaise on our road to Kew, where was a monthly meeting called the 'medico-botanical.' I can hardly describe my feelings on receiving this information. I said to him, 'I hope, Sir Everard, you have not destroyed those ten volumes relating to the gallery and Mr. Hunter's lectures?' he replied that he had; and then I mentioned to him perhaps twenty others, of which I had a very perfect recollection (and I felt that all the hopes which I had entertained were entirely frustrated and destroyed; my life had been spent in the service of that collection, and I hoped to have lived to see those papers beneficially employed;) but he told me that they were *all gone*, and then said I to him, 'Well, Sir Everard, there is only one thing more to do, and that is, to burn the collection itself.' We had very little conversation after that for a very considerable time. I considered that an irreparable injury had been done to the collection. That week Sir Everard had received back from the printer the last proof of his second volume of Lectures on Comparative Anatomy; and I knew *that he had used those papers very largely in the composition of that work.* Four or five days afterwards I made a communication on the subject to the members of the council, first communicating it to the chairman of the board of curators, Sir William Blizard, in private. A board was summoned very quickly in consequence of that, and I was called before it, but Sir Everard was not. Having learned during my conversation with Sir Everard that he had not destroyed all the manuscripts relating to the pathological prepara-

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Clift became wholly independent of the thief, the board of curators, that worse than incapable body, were made acquainted with the want which led to the discovery of the robbery.

*Yet all this time the farce of urging the thief to "draw up a descriptive catalogue," underwent a quarterly performance by the trustees of the museum!

tions, for (he had said, 'I have some of those') at this board of curators I mentioned that circumstance, telling them that some were according to his own confession still in existence, and they of course did not need my advice to apply to Sir Everard for them."

219. "Will you refer to any memoranda you have on the subject, and state, as nearly as you can, what the papers were that were destroyed?"

"Among them were nine folio volumes of dissections of animals, viz: Vol. I. Ruminants Vol. II. Animals *sine cœco*. Vol. III. Monkey and its Gradations. Vol. IV. Lion and its Gradations. Vol. V. Scalpris Dentata. Vol. VI. Anatomy of Birds. Vol. VII. The Tricolia. Vol. VIII. Anatomy of Fishes. Vol. IX. Anatomy of Insects. There was one volume on the Natural History of Vegetables. There was also a great number of fasciculi, among which were the following:—Introduction to Natural History; numerous Physiological Observations; Comparative Physiology; Comparison between Man and the Monkey; On Muscular Motion, being subjects of Croonian Lectures; Effects of Extracting one Ovarium upon the number of young produced; Experiments on Ewes, with a view to determine Impregnation and Uterine Gestation; On Monsters; On the Skeleton; Dissection of the Tapir; Dissection of the Armadillo with nine bands; Animals from New Holland; Piked Whale, Bottle-nosed Whale, Fin-backed Whale, Porpoise; Worms in Animals of the Whale Tribe; Bell-Barnacle; On the Eel; Anatomy of the Holothuria; Anatomy of the Siren of North America; Account of the Unicorn Fish from Hispaniola; The Earth Worm; Progress and Peculiarities of the Chick; Description of Rymsdyk's Drawings of the Incubation of the Egg; General Observations on Insects, the Bee Tribe, Humble-Bee, Wasp, Hornet, and Beetles; Anatomy of the Silk-Worm; Anatomy of the Moth; Red-piped Coral. On Fossil Bones, two parts; Observations on Surgery; Observations on Scrofula and Cancer; Lectures on Principles of Surgery; Cases with *post-mortem* examinations; Cases where no *post-mortem* examinations were obtained; Two cylinders of Cases, written out separately and fairly. Two volumes in folio were recovered by the curators on application to Sir Everard, perhaps a tenth part of the whole of the papers relating to the preparations in the collection."

220. "Do you find any thing in reading Sir Everard Home's Lectures that in any way *supplies the loss* as regards the descriprion of preparations in the museum?"

"Sir Everard's is a mere general description; it seldom refers to particular specimens."

221. "Were you old enough, at the time of Mr. Hunter's death, to have any conversation with him on the subject of these papers, so as to know whether he attached any value to them?"

"I was Mr. Hunter's apprentice from February, 1792, till his death, on the 16th of October, 1793. During that period I was employed every evening in writing for him, and many of those manuscripts were in my own handwriting. I had no conversation with him on the subject, of course, because I was but a boy, and had merely to transcribe what he gave me to transcribe, in the same room with him. I was employed in taking care of the collection all day long, and frequently transcribed portions which were in loose notes, into volumes which he would take down for that purpose."

222. "Did you ever hear any hint from him of his wish that those papers should be destroyed?"

"Never; nor did I ever hear, either from Sir Everard Home or from Dr. Baillie, his co-executor, any hint whatever of any directions that Mr. Hunter had given for the destruction of the manuscripts."

223. "Will any industry that you or your fellow-laborers at the museum can bestow upon it, ever repair the loss which it has sustained by the burning of those manuscripts?"

"No, it is impossible. Some of the preparations have been rendered wholly useless by the loss of the accompanying description. It is very frequently necessary to re-examine the organs of animals that are daily presenting themselves to us, in order to throw light upon the specimens in the museum, and to prove what they really are; because many of them (the descriptions being absent) are utterly unknown. The nature of many of them has been discovered, as it were, by these means. We preserve the unknown preparations, and endeavor to discover by degrees what they are. There may be, of all kinds *a thousand*, of wet and dry, yet remaining, that are unexplained. The loss is irreparable in the department of comparative anatomy."

224. "When the trustees wrote to Sir Everard Home, did they require an account from him of the papers he had destroyed?"

"No, not then, but they did so on receiving his answer, saying, *that he had destroyed them entirely*, that having used them for the purposes of the collection for so many years, and, AS A WINDING-UP OF HIS EXECUTORSHIP, he had, according to a promise he had made to Mr. Hunter, destroyed them. The letter was so worded that you would have taken it for granted that they were *all* destroyed. This letter was read to me by the trustees, and I said, 'Sir Everard told me that he had not destroyed them *all*;' and therefore that they ought to write to him to ask for those that had not been destroyed. They said, '*How can we do so?* because his letter expressly states that they are all destroyed.' To this I replied, 'Then Sir Everard must have told me an untruth,' and then I told them what parts he had assured me were *not* destroyed. They wrote to him, accordingly, for those papers, AND HE SENT THEM *such as he pleased!* A very small parcel, in the first instance, was sent by Sir Everard to the trustees. Those papers were put into my hands, to see how far they were applicable to the purposes of describing the collection. Some of them referred to others that *I knew did exist*, but were only a very small proportion of those that I had a perfect recollection of. A second application was then made for more, and that extracted a much larger parcel. I am not sure whether there was any further application."

225. "Were any applications made to *his executors*, to inquire whether there were any remaining papers of Mr. Hunter in their possession?"

"Yes, with considerable success. The volume that was mentioned, respecting vegetables, referring to a part of the collection, from which I have been able to explain some of the vegetable preparations in a way that we could not have done without them, was recovered; and there were some that were possibly Sir Everard Home's, that also threw light upon many of the preparations. These were *duplicates of some of the papers of Mr. Hunter*, yet at the time of the last recovery of papers form Sir Everard Home himself, he stated *that those were all the papers he had retained in his possession.*"

226. "Do you conceive that if Mr. Hunter had given him directions to destroy his papers after they had been made available for the pur-

poses of the collection, Sir Everard really had *complied* with those directions, and had extracted from them all the valuable information they contained relating to the collection, and applied it to illustrate the preparations in the museum?"

"Certainly not."

227. "Therefore, according to his own statement, he had destroyed them *before he had complied with the directions of the testator;* if, indeed, those directions were ever given?"

"*Certainly.*"

228. "Did Sir Everard Home ever state where and when these directions were given by Mr. Hunter?"

"I had heard it stated, but I do not know by whom, that it was when Mr. Hunter was dying, *which I knew could not be true*, because I was the last person in his family who saw him alive, and I knew that Sir Everard *was not present at his death.* This I told Sir Everard at the college, five or six months after the conversation which took place on our way to Kew, and I told him so in consequence of what had passed between him and the trustees."

229. "Was it in the presence of others?"

"No, it was alone, it was a very rambling conversation, an hour and a half long, I believe."

230. "Did Sir Everard begin the conversation with you, or did you begin with Sir Everard?"

"He came to the collection, and it was the only conversation I ever had with him after the first conversation on the destruction of the papers. It was entirely on the subject of those papers. He told me, that I must have known that the papers were not fit to be submitted to the public eye, because they were in such a state. He said the spelling was not very correct. I said, that he knew very well that if this catalogue ever was to be made, the *labor* of it must come to my share, and that I did not consider my eye to be the public eye; and I added, that he would have been welcome to all the credit of the catalogue, so that the thing had been but done: but he said that directions were given to him by Mr. Hunter to destroy the manuscripts when he was dying; and I said, 'That is impossible.'"

231. "What was his remark upon that?"

"He made no answer."

232. "Did he leave the impression with you that such a direction had been, or that it had not been given?"

"If he had sworn it, I would not have believed it. The duration of Mr. Hunter's illness was not a minute. He died of a disease of the heart."

233. "Were you employed by Sir Everard in making drawings for his papers in the *Philosophical Transactions*, or in other publications?"

"All my life, from the time that I was first acquainted with him, till the time of this unfortunate occurrence,—that is, till the time of the conversation on the way to Kew."

234. "Did that conversation entirely interrupt the friendship that you before had with Sir Everard?"

"Entirely; I had no other cause for its interruption."

235. "Why do you suppose that the manuscripts of Mr. Hunter were used by Sir Everard in preparing papers (for the Philosophical Transactions or other publications) bearing his name?"

"*Because I frequently transcribed parts of Mr. Hunter's original*

papers and drawings into the papers which were to appear in Sir Everard's name."

236. "When he made his first avowal of the destruction of the manuscripts, did he appear to have made it involuntarily, or how did it happen?"

"It was, I believe, to see how I would take it; but though there had been a regular application to him *every quarter* to complete the descriptive catalogue, yet as I said before, the papers were not destroyed until immediately after receiving the last proof sheet of his second volume."

237. "*Was any meeting held for censuring Sir Everard Home, o for excluding him from the council?*"

"NOT TO THE BEST OF MY KNOWLEDGE."

238. "Did he continue to be a member of the council until near the day of his death?"

"I am not sure. He died a trustee of the museum."

239. "Is there any thi g further relating to this deplorable transaction that you wish to state to the committee?"

"I do not recollect any thing further. I do not know that the council of the college ever came to any resolution on the subject of Sir Everard's conduct, nor have I ever heard that the nature of the loss was communicated to the government by the trustees. The destruction of the papers happened immediately previous to the death of Sir Everard Home's co-executor, Dr. Baillie, perhaps a month or two, but he was so ill that it was impossible that he could have interfered, even had he known of the circumstances. He might have been considered as not acting in the business; because Sir Everard very rarely communicated with him. Sir Everard was Mrs. Hunter's brother, and took all the active part. No communication was made to Dr. Baillie on the subject previous to his death."

240. "Has not the want of access to the manuscripts descriptive of the preparations, very greatly added to the time and labor required to describe the collection?"

"Most unquestionably it has, in a very material degree. Moreover I felt that if I could describe the collection in Mr. Hunter's own words, the public would be better pleased with it, whether Mr. Hunter was right or wrong. It might have been altered or improved by any one who thought himself competent to do so; as it is, we are obliged to trust to our own resources and knowledge, instead of that of Mr. Hunter."

241. "How was it that you became so intimately acquainted with the nature of Mr. Hunter's manuscripts that were destroyed?"

"After Mr. Hunter's death till 1806, when the collection was transferred to the college of Surgeons, I had the key of the cases which contained them, and was anxious to make myself acquainted with the nature of their contents, as they related chiefly to the preparations. I had also, I may say, no other books to read at that time; so I frequently availed myself of the opportunity to read them, and make extracts from them, and I know that they related, many of them, entirely to the preparations in the museum. Thinking there was a great deal of useful information in them, I made large extracts from some of them, and have been instrumental in preserving, in substance, by means of those extracts, I hope nearly half."

242. "Did Sir Everard Home ever make to the curators or trustees

of the museum any denial of the statements you made to them as to the number and contents of the manuscripts?"

"No, not a single denial."

243. "Up to the destruction of the manuscripts, on what terms of intimacy were you with Sir Everard?"

"I looked upon him as one of my oldest and best friends; to that hour we had never had the slightest disagreement, but this unfortunate event put a stop to that intimacy: for I considered the obligation which I owed to the memory of Mr. Hunter, to the trustees, and to the curators, superior to any private obligation which I might have been under to Sir Everard Home, and that was my only object in mentioning the subject, which I wished it had never been necessary for me to disclose."

It is a matter of deep regret, that one who was calculated to adorn science, and shed a lustre on the profession of which he was a member, should have been so destitute of self respect, as to violate a trust, than which none more important could have been committed to his hands,—the property, reputation and honor of a deceased brother.

The inquiry has been raised—Why was this disclosure not made long since, and during the lifetime of Sir Everard? We reply that, according to the testimony of Mr. Clift, it was made to the trustees of the college as early as 1823, but there is no evidence of their having made any report to the government on the subject. The matter was permitted to sleep in silence till the parliamentary committee instituted their inquiries, by which the conduct of Sir Everard was brought before the gaze of the scientific world. The dishonorable employment of the manuscripts of John Hunter for purposes of self-aggrandizement, will consign the name and the memory of Sir Everard Home to merited obloquy and contempt.

C. R. C.

VIII. *The Physiology of Respiration and Chemistry of the blood applied to Epidemic Cholera.* By BENJAMIN F. JOSLIN, M. D., Professor of Natural Philosophy, in Union College, N. Y. [Extracted from the Transactions of the Medical Society, of the state of New York.]

Dr. Joslin's essay contains the history of a series of original experiments on the influence of different agents on the

color of the blood, which we propose to present to our readers in the following extract:—

Experiments on the Blood.—Great importance being generally attached to the deficiency of salts in cholera blood, and the following experiments, on blood nearly or exactly normal, differing perhaps in some respects, from those made by others, they may be worth recording. When disease existed it was slight, especially as to its effect on the blood. To prevent mistake it is necessary to state, that in all these experiments, I describe the color of the blood as seen by reflected light.

Venous blood was drawn from the arm of a man who had slight symptoms of fever, with gastro-intestinal irritation. The blood coagulated promptly and completely. The clot, as is usual, was dark colored beneath the surface. This was reddened by applying to it carbonate of soda in powder. On laying a lump of it on a dark portion of the coagulum, the surface in contact with the solid alkaline carbonate, though covered by it, was rendered of a scarlet color much more rapidly, than another portion wet with a saturated solution, and exposed as freely as possible to the air. The liquid which drained from the coagulum to which the dry soda was applied, as also the solution applied, was almost instantly rendered of a bright red color, not only at the surface, but throughout the interior, where no air was present, except the small quantity contained in the blood and in the solution. These experiments led me to suspect, that this alkaline salt has the property of changing the color of *any* dark venous blood to scarlet, and that less air is requisite than is generally believed.

The blood on which the next experiments were made, was drawn from one of the large veins of a hog, and received directly into four one ounce phials, which were completely filled. Three were instantly corked, and one left open. Of two of these which were corked, I had, before receiving the blood into them, put two scruples of carbonate of soda into each. One of these was then uncorked and immediately put under the receiver of an air pump, which was kept exhausted for ten or fifteen minutes, during which time considerable gas escaped from it. On removing it from the receiver, about one drachm of water was added, to compensate for the gas and blood which had escaped in the form of froth, and thus to exclude atmospheric air. When filled, it was immediately corked, and all the corked phials shortly after sealed, and kept at a temperature of about 60 deg. Fah. The color of both portions into which the soda had been put, was changed to scarlet, at the inferior part which was in contact with the salt or near it; but that from which the gases had not been removed, was much brighter than the other, and its coagulum had much less consistence. The two others without soda were sensibly alike in color, except that the one exposed to the air was reddened at the surface. The blood reddened by the soda, began, after an hour or two, to become sensibly darker, but after three hours, was still much brighter than the other, and that from which the gases had not been exhausted, was but little brighter, at that time, than that which had not been placed under the receiver. The next day, the order of brightness of the blood of the three closed phials, was completely reversed.

In the next series of experiments, a portion of florid blood drawn from the epigastrium of a man affected with slight chronic gastro-enteritis, was put into hot water. The coagula were blackened by it as usual. It has been asserted, that in such cases, the water extracts those salts on which the florid color depends. The following experiments proved, that the change of color is produced in such cases, neither by the extraction of salts nor by the extrication of oxygen. Half an ounce of the same blood in its florid state, was put with an equal quantity of water, into an ounce phial and boiled in a water bath. Both the liquid and solid parts became of a light olive color and a gaseous substance escaped. To determine the effect of a lower temperature, and to remove the ambiguity occasioned by the escape of the gases, as well as to determine the combined effect of heat and an alkaline salt on serum and red globules when separated from the fibrine of the crassamentum, two ounce phials were filled with the serous and aqueous liquid, in which the clot had been blackened, and to which it had imparted a florid color. A scruple of carbonate of soda was put into one of them, and both were then corked and immersed in a water bath, which was gradually heated to 150 degrees. At

this temperature both became opaque; that which contained the soda soon becoming perfectly black as seen by reflected light, the other a dull brownish red. A half ounce phial was next filled with the same florid liquid, corked, sealed and immersed in water at 150 deg. Fah. It soon became opaque and brownish. A scruple of carbonate of soda was then added, which immediately rendered it black. Another portion was blackened by dilute nitric acid without heat. The blackness produced by the carbonate of soda remained after filtering through paper and exposure to air. A month afterwards, the black color remained unchanged, and was not affected by reheating the liquid to the temperature formerly employed. It is a curious fact established by the above experiments, that carbonate of soda produces the same black color in sanguineous coloring matter diffused through diluted serum and heated to 150 degrees Fah., that nitric acid does at 60 degrees. As a change of temperature is thus shown to modify and at length to reverse the effect produced by the salt, it is not improbable, that a repetition of the experiment with different salts and at different temperatures, might lead to interesting discoveries and suggest improvements in venous injection. It appears from these experiments, that the effect of saline substances on blood at one temperature, cannot be inferred from experiments made at another. These experiments moreover prove that the blackening of crassamentum by hot water is not dependent on the extraction of its saline matters; and also, that the change of color produced in sanguineous coloring matter by heat is not the result of the extrication of oxygen or any other gas. Indeed this change of color was produced by heat, when the phials were filled and perfectly closed, and was more remarkable when the saline matter exceeded the normal quantity.

Some of the preceding experiments were made (as has been stated) on serum and coloring matter only. In all the following experiments all the parts of the blood were in the same proportion as before its extraction from the vessels. Two one ounce phials full of venous blood, after having stood corked and sealed exactly six months, the one without any thing added, and the other with five grains of carbonate of potash, were heated in a water bath. At and above 140 degrees, considerable gas was evolved, but no change of color was produced by heating them to 155 degrees; the seals being then broken by the force of the gas and vapor, the experiment was discontinued. In the next experiment, five grains of carbonate of soda were put into a phial with ¼ ounce of blood extracted by cupping. Heated to 142 degrees it was a little darkened and at 150 degrees became black. A small portion seen by transmitted light appeared of a dark yellowish green color. The coagula appeared to have been rendered somewhat fluid. On raising the temperature, little change of color or consistence was observed till it reached 170 degrees, when the whole was found to have lost its fluidity and to have assumed the consistence of currant jelly. This consistence was not again diminished by cooling, but on the contrary increased. A heat of 150 degrees had rendered it black, whilst a heat of 170 degrees had coagulated the whole, without however giving it the tenacity or color of the original coagula. Even when cooled it had little of the elasticity of crassamentum, though it had no fluidity. No gas was evolved during any part of the process. The next and last experiment was made with three one ounce phials full of blood, which may be distinguished as A, B and C. A, was venous and arterial blood obtained by cupping, and simply sealed. B and C venous blood, the two portions exactly alike. To B, grs. v. carbonate of soda were added; to C, grs. v. of the carbonate of the same alkali. All being sealed were exposed to the same heat.—The following were the results: The color of A, was not sensibly changed till heated to about 170 degrees, when it underwent a kind of coagulation, assuming a consistence and elasticity like that of a solution of glue when cold, its color simultaneously changing to an olive grey. B became black at 150 degrees, (its color beginning to change at about 148 degrees.) This blackness extended only as far as the previous reddening influence of the carb. sod., the rest remaining of the original dark red color. C was not in the least darkened at 150 degrees; but where the bicarbonate had come in contact with it, it still retained that excess of brightness which the salt had communicated, till heated to some point between 160 degrees and 170 degrees, when it became dark, but not as black as B. From a comparison of B and C, I infer that that salt of soda, which contains most carbonic acid requires a higher temperature to render blood dark, and blackens it but imperfectly even at a higher temperature. From these last experiments, and from a compari-

son of those on A, with similar ones which have been described above, and which were made on the same kind of blood, I infer that the carbonate of soda with heat, blackens both venous and arterial blood. The coagulation of the mixed blood at 170 degrees was effected by heat alone. These last experiments then, though made under circumstances which developed some new and curious phenomena, tended to confirm and render more general the preceding conclusions in regard to the blood."

After this statement of his experiments, our author proceeds to insist, on a close analogy between the state of the blood in Epidemic Cholera and asphyxia, in both of which, he believes, it is loaded with carbon, and has its coagulability much diminished, or entirely destroyed. He does not attach much importance to the loss of serum, and is from principle altogether sceptical as to the beneficial influence of saline injections into the veins. Of the manner in which this highly carbonaceous condition of the blood is brought about he does not pretend to be satisfied, but presumes the remote cause to exert itself upon the lungs. The prominent symptoms and fatal termination of the disease may, he thinks be fairly attributed to this altered condition of the blood, disturbing and prostrating the different functions, as they are annihilated in asphyxia.

Under the head *aetiology*, our author recognizes the existence of an unknown ærial cause as predisposing to the disease, and the following auxiliary or exciting causes.

"1st, Heat; 2d, Long-continued and violent exercise; 3d, Alcohol; 4th, Fasting; 5th, The depressing passions; 6th, Night; 7th, Vegetable diet. To these should probably be added—Cutaneous Filth."

The whole of these influences, he supposes, co-operate with the unknown agent in diminishing the oxygenation or decarbonization of the blood, and subjoins to each an argument to show that its effect is of that kind.

The *modus agendi* of this undiscovered cause may be such as to lead to the same state of the blood, carried to a higher degree than the common causes assigned by Dr. Joslin could have produced; or it may have acted in some other mode, so as to give to defective oxygenation from the conditions he has assigned, a noxious influence greater, than it would otherwise have effected. However, this may be, we perceive the neces-

sity of assuming the existence of such a cause, and of placing it in the rank of a principal and predisposing influence. It is far from cur intention to speculate on the nature of this cause, but, of course, we believe it to be connected with the atmosphere; and it may, perhaps, consist in one of the following conditions—first a diminution in the proportion of oxygen; a defect insisted upon by us in the last number of this journal, but which, it must be confessed, is only a hypothesis;—second a modification of the electro — magnetic state of the air — likewise hypothetical;—third, the presence in the air of some noxious agent. In adopting either of these suppositions, we are, however, met, in a palpable manner, by the fact, that Epidemic Cholera is a progressive disease—that it traveled from India, within the tropics to Russia where it spread almost at the arctic circle; that it passed over to Great Britain, thence to Canada, thence to the shores of the Hudson, and the Ohio; that advancing rapibly to Louisiana, it extended round the Gulf of Mexico, and soon after invaded South America and the West Indies; that from the north and east of Europe it descended upon the south-western kingdoms of that continent, and still lingers on the shores of the Mediterranean. To such a progressive spread among the nations of the earth we consider it impossible at present to reconcile any of the theories we have cited.

Our limits do not permit us to give as full an analysis of Dr. Joslin's paper as its merits deserve. We have not for some time looked over a paper on the physiology and pathology of cholera, that was written in such a philosophical spirit, and presented as many sound views of the subject; or as sensible an application of the science of chemistry to that of medicine. We hope the Medical Society of thestate of New-York will long continue to favor the profession with such papers.

III.—ORIGINAL.

The Western Journal.

E Sylvis Nuncius.

Cincinnati, October 1, 1835.

Art. I.—Absorption of the Liquor Amnii—Descent of the Gravid Uterus.

Every obstetrician must have met with cases in which, at the time of parturition, there was little or no discharge of liquor amnii. An instance of this kind lately occurred in our own practice. The patient nearly 40 years old, not before in a state of gestation, was so large at the beginning of the ninth month, as to excite the expectation of twins. Two or three weeks before delivery, she began to diminish in size, and in a few days was reduced to a degree that struck her as remarkable. As the labor advanced, the aqueous tumor in front of the head of the fœtus, was barely sufficient to show the integrity of the membranes, and when we ruptured them scarcely an ounce of fluid escaped; there was no perceptible discharge during the subsequent pains, and none followed the escape of the fœtus.

The absorption of the liquor amnii, to a greater or less extent, in the latter days of gestation, is no doubt a constant physiological fact, of which our obstetrical writers seem to

have taken very little notice. The diminution of size in the uterine tumor is generally ascribed to the subsidence into the pelvis of the head of the fœtus; but such a sinking down, if in fact it occurs at all, must be too little to explain the great reduction of size in the abdomen:—that reduction we are convinced is chiefly the effect of absorption. As the remaining waters would of course be accumulated in the lower part of the uterus, the size of the patient, measured round the ossa ilia, would, except in the few cases in which the absorption was perfect, be nearly the same as before, while her circumference between the umbilicus and epigastrium would be lessened, thus giving the appearance of subsidence into the pelvis.

The objects of nature in this absorption seem to be threefold. First, by taking off the tension of the uterus that organ can accommodate its figure to the descent of the head, into the superior straight of the pelvis, which it could not do when distended:—Second, the diminution favors uterine contraction, in the same manner as removing, with the catheter, a portion of urine, in ischuria, enables the bladder to contract on the remainder, and empty itself. The immediate effect of the escape of a portion of the liquor amnii during labor, in augmenting the expulsive power of the uterus, is familiar to all practitioners, and depends on the same cause. By the way, we feel inclined, as the opportunity here offers, to take exception to the rule, copied from one system of obstetrics into another, that the membranes are not to be ruptured till very near the close of labor. Whenever the uterine tumor is very great, and the expulsive pains inefficient, a comparatively early rupture of the membranes facilitates the labor, by enabling the organ to contract with greater power. But let us return to the third and last end of nature, in the absorption of the liquor amnii. This is the greater safety of the mother, whose, exhaustion, immediately after delivery, will, *cæteris paribus*, be indirectly as the amount which then suddenly escapes from the uterus.

II.—Death following the operation for Congenital Cataract.

In the year 1834, a little girl, five or six years old, was brought from Indiana to the *Cincinnati Eye Infirmary*, with congenital cataract. She was apparently of infirm constitution, and but little developed in her intellectual functions. The needle, for the performance of Mr. Sanders' operation, was introduced through the cornea, and, as the pupil was broadly dilated, the iris was not touched. A slight laceration of the capsule of the lens of each eye, was followed by its breaking up, and the immediate appearance of a black color. Thus the operation was attended with the least possible violence. Nevertheless, she very soon fell into a deep state of nervous irritation, which involved the brain, and in a few days proved fatal.

III.—Comparative Pain and Constitutional Affections from Conching and Extracting the Cataract.

The people, and many practitioners, seem to have an opinion, that conching is attended with less pain and constitutional disturbance, than extraction; but from considerable experience we are convinced that this is not the fact. The introduction of the needle through the scleroliæ is generally attended with greater suffering at the moment, and a much deeper state of nervous irritation, with nausea, than the incision of the corneo; and the pressure of the dislocated lens is more severely felt by the patient, than its escape from the eye. These facts, with many others, lead us to prefer extraction to depression, provided, of course, that the state of the cornea and anterior chamber be such as to favor the former operation.

III.—Safety and success of extracting the Cataract in old age.

Very lately we extracted a cataract from the eye of a woman sixty-four years old, which was followed by no more inflammation than was requisite to adhesion, although she had not been subjected to any previous preparation, either medicinal or dietetic. Last year we performed a similar operation on a man seventy-five years of age, who recovered in a favorable manner. Four or five years since, we performed the same operation on a man seventy-four years old, whose eye became inflamed for a while, but he had long been subject to ophthalmia. About the same time, we performed the same operation on a man in his eighty-sixth year, who in a week was able to see, and recovered with the least possible inflammation. From these cases we may infer, that violent and obstinate inflammation is even less likely to supervene on this operation, in advanced than middle and early life; a fact that should induce the friends of the aged, not to abandon them to the gloom of total blindnes in the evening of life.

IV.—Medical Department of Willoughby University, on Lake Erie; and Medical College of Louisiana, New Orleans.

It was our intention to announce to our readers the plan of organization, and the names of the professors, of these young institutions, both of which will be in their second session, with a more perfect arrangement, the ensuing winter; but, at the moment when we would refer to their circulars they are mislaid. In our next number we hope to give a more extended notice of them.

V.—Dr. Julius, of Prussia.

We have lately derived much gratification from several days' intercourse with this enlightened physician and civilian,

sent to the United States by the king of Prussia, to inquire into the state of its institutions, especially its penetentiaries and other prisons. Dr. Julius is one of the editors of a medical periodical entitled "The Journal of Foreign Medicine," published in Hamburg, which we shali hereafter receive in exchange for this journal, and from which our learned friend, Professor Gross, will make such translations as may be interesting to our western readers. Dr. Julius is a gentleman of great observation, and having directed his attention to the state of the medical profession in the United States, will, it may be expected, on his return, furnish us with some sound critical advice, on our plans of study and systems of medical police. In a free conversation with him on the relative scope of studies in Prussia and the United States, we could not ward off a feeling of mortification, at the limited and superficial character of the latter compared with the former. We do not know that Dr. Juluis intends to write a book of travels, but we hope he will at least give us a memoir in his periodical, on the state of medical instruction and professional learning in America.

VI.—Study of General and Pathological Anatomy.

As far as we know, the first chair of *General and Pathological Anatomy* instituted in the United States, is that of the new medical department of the Cincinnati College, which was announced in our last number. Dr. Gross, the professor, a gentleman whose course of studies and mental temperament, peculiarly qualify him for the duties of such a chair, has already made a valuable collection of morbid specimens, which he is augmenting daily. They will be arranged in the College edifice, and at all times open to the inspection of the members of the profession; from whom, in turn, the Doctor will be gratified to receive such specimens of a similar kind, as occasional *post mortem* examinations may put in their possession. They should be accompanied with an account of the symptoms under which the patients, from whom they

were taken, labored. To fit a specimen for transportation, it should be thoroughly washed with cold water, and then corked or sealed up, in a wide mouthed bottle, entirely full of strong whiskey or alcohol. Dr. G. will also be thankful for specimens taken from brute animals, which should also be accompanied with notices of the fatal symptoms.

In his teachings of General Anatomy and Physiology, the Professor intends to dip into the Comparative, and is desirous of enlarging his collection of Comparative Osteology—especially of acquiring the skeletons of our indigenous animals. In consequence of the death of a large lion, in one of the traveling menageries, which visited our city last year, he has obtained the bones, and several of the soft parts of that animal; and being desirous of comparing the former with bones of our American panther, would be glad to procure them.

VII.—Analysis of Minerals and Mineral Waters.

Professor Rogers, and Adjunct Professor Riddell, of the medical department of Cincinnati College, will be prepared in the ensuing winter, to analyze, with accuracy, any of our native minerals and medicinal waters that may be sent to them. With every mineral specimen there should be transmitted a specimen of the rock in which it was embedded; and mineral waters should be put up in sealed bottles, entirely filled, and corked under water.

IX.—Professor Jameson.

This gentleman, in accepting the chair of *Surgery*, in the Cincinnati College, has transferred his family to this city, for permanent residence. Those citizens of the west and south, therefore, who may wish to avail themselves of the ripe experience of this eminent surgeon, will have an opportunity of visiting him in Cincinnati; and such physicians as desire to purchase his Medical Journal, in two volumes, may find in our book-stores.

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X.—Intermittent Fever in the West.

Intermittent fever appears to be prevailing extensively in the west. Why does not this malady appear in town as well as country? In Cincinnati we seldom have a case unless the person had visited the country. We speak of the thickly settled portions of the city—for the suburbs, especially the western, in the direction of the low alluvial shores of Mill-creek, have been partially affected. We confess ourselves unable to assign any satisfactory reason, for the exemption of cities from this disease; for there are, certainly, within most of them, various sources of malaria; and malignant remittents occasionally occur, even as epidemics.

As far as we have heard, the intermittent fever of the present autumn is mild; but there are exceptions, some of which, through the politeness of the attending physician, Dr. Ridgely, have fallen under our own observation. As these cases were of a remarkable character, we shall insert a notice of them.

Burlington is a small village on the Ohio river, in our own state, nearly opposite the mouth of Great Sandy, which separates Virginia from Kentucky. A family by the name of Cox, resided one mile below the village, on the north bank of the Ohio river. The shore is high, and exempt both from alluvial accumulations and collections of water; but on the opposite side of the river, above the mouth of the Big Sandy, there are several large ponds. The people on both sides of the Ohio, including those of the village of Burlington, were generally affected with intermittent fever. Among the rest, Mr. Cox and every member of his family, amounting in all to eight persons, were taken down. He, himself, in the course of the disease was seized with convulsions and delirium, of which he died. One of the children, laboring under the fever, became affected with symptoms of epidemic cholera, and died. Another laboring under the same fever, experienced an attack with convulsions, like the father, which terminated in hemiplegia, from which, however, it has nearly recovered. All these events happened at the same place. Soon afterwards the remaining members of the family removed to Cincinnati,

and fell under the care of Dr. Ridgley. One of the children, a boy four or five years old, when the Dr. first saw him, appeared to be coming out of the cold stage. He was able to sit up in the bed and converse rationally. But soon after the Dr. left the house, he said he was dying, and in fact expired —having complained of severe pain in his bowels, a symptom which existed in the paroxysm of the preceding day. Not long afterwards, a daughter, two or three years older, laboring under the same form of fever, was attacked with convulsions, accompanied with hemiplegia, and after several repetitions, throughout the intervals of which she remained senseless, she expired. Two other children and the mother are recovering. One of these children, according to the statement of the mother, had a paroxysm of the fever, when it was but three days old. Of the two that died in the city, Dr. Ridgley was permitted to examine the body of one only, the boy, but had not an opportunity of inspecting the brain or spinal marrow. The mucous membrane of the stomach and bowels was free from inflammatory lesions. The liver was unusually firm, and of a leaden color. The spleen was dark colored, engorged, and enlarged.

The whole family had been treated, before they came to Cincinnati, with the sulphate of quinine, and bloodletting, both general and local, had been omitted. The Dr. and myself are of opinion, that the whole, at first, required the lancet; and suppose that to its omission, and the early and empyrical administration of the sulphate of quinine, the sinister termination of most of them might be fairly attributed. One of the children who is recovering, has sluggish bowels and a tumified abdomen. The treatment of this case is by purgatives, alternated with the sulphate; by which purging we propose to divert from the brain, and prevent a lesion of that organ, while the sulphate exerts its specific action in arresting the paroxysms.

When intermitting fever is epidemic, it can scarcely be doubted, that many patients are lost by the premature and indiscriminate administration of quinine, a medicine which affects the brain much more than the Peruvian bark in substance.

XI.—Medical Observations on certain parts of the Delta of the Mississippi.

Locust-Grove, Assumption, La., Sept., 1835.

Dr. Drake—Dear Sir,—In the last No. of your Medical Journal I noticed a communication from Dr. Cartwright, of Natchez, giving it as a fact, that "certain parts of the Delta of the Mississippi are proverbially exempt from autumnal diseases," or in other words, to the bilious fevers which other parts of said Delta are subject to. Now, from actual observation on the spot, I would beg leave to differ with the Dr. as to the fact of said exemption, and as to the cause of it, in case it did exist.

I will, however, admit, in the first place, that said section of country is comparatively healthy, with respect to some other portions, situated more interiorly; and much more healthy than might be supposed from its locality—being situated, as the Dr. justly remarks, on bayous without current, and surrounded by swamps and lakes, extending into bogs, that communicate with the Gulf of Mexico. But after admitting its comparative healthfulness, it is far from fact to say, it is not yearly subject, more or less, to bilious fevers of different grades, owing, of course, to difference in seasons, as hot and cool, wet and dry, etc. A wet summer throughout, for instance, is universally healthy; but a great quantity of rain at any time in the summer, followed by a few weeks of hot sultry weather, is as universally followed by severe bilious fevers.

In taking a view of the Dr's. position, we would be led to suppose that our autumnal diseases are dependent on the stagnant waters existing in lakes and bayous; and a vegetable growth on their surface, would make such localities perfectly healthy. This position I conceive to be utterly fallacious, and not tenable by fact or observation. As before observed, whilst the earth is kept constantly saturated with frequent showers of rain, and the swamps are kept full of water, we are always healthy: but when such state is succeeded by great and continued heat, drying away the water from the earth and swamps, fever is certain to succeed. This state of the

atmosphere, *consequently* filled with miasmata, is not, I conceive, materially mitigated by the vegetable growth spoken of by the Dr. as covering the surrounding waters, as it is only one of nature's means of purifying *them*, and not the atmosphere any more than any other vegetable growth. The principal cause of the comparative healthiness of the region above alluded to, including the Bayou Lafourche, I conceive to be, its contiguity to the Gulf of Mexico, and the constant prevalence of winds, mostly in a southerly direction, throughout the summer months.

I am sorry, therefore, to acknowledge, that even if the Dr. gets a vegetable growth to cover the lakes and bayous, in Concordia, I have my doubts, if the ills of humanity will be much mitigated thereby. It is not, however, at all probable, that they can be made to grow; on account of the overflown waters of the Mississippi coming in contact with the lakes and bayous, situated in that section, every spring.

I would make a single remark on another subject mentioned by the Doctor, viz: as to the best locality for persons predisposed to phthisis pulmonalis. I believe it is now generally admitted by the most enlightened of the profession, that localities situated immediately adjoining the sea, are not the best adapted for persons predisposed to this disease—more especially high and dry situations. The reason why I would reject such localities, is that the atmosphere is too inflammatory, if I may be allowed the expression, or too stimulating for the lungs of such patients. For this reason I would recommend the lowest and consequently the dampest situation that can be found, and as far south as possible, so as at the same time to be clear of the immediate influence of the sea breeze. Such locality can be found on the upper and middle portions of Lafourche—being in the latitude of New Orleans. From seven years observations I hardly known an original case of consumption.

Respectfully, your obd't servant,

W. KITTREDGE.

XII.—Geological Surveys.

Several of our state legislatures have, within the last few years, made provision for geological surveys of their respective territories; and some of them have been finished, while others are in progress. This is a policy which we hope, ere long, to see adopted by every state in the Union. As citizens and men of science, we feel a deep interest in such surveys; and as physicians we desire to see them executed. It has long been known, that some diseases are peculiar to certain countries, and that others are strikingly modified by soil and water, not less than by climate. A geological survey is the basis of all medical topography; and until such a survey of the different states is made by competent persons, there will be no safe and solid foundation for their medical history. It is by these examinations that we acquire a knowledge of the chemical constitution of the soil, and its substrata, and their influence on the waters of wells, springs, and streams; the extent and composition of alluvial and diluvial deposites; the elevation of the whole country above the level of the sea; its depression below the adjoining mountains, and the height of its hills over the intervening plains; the distance and bearing of the alpine ranges which modify its temperature, and the course of its valleys and gorges, which exert so much influence on the direction and velocity of its winds. With every survey of this kind there should be connected, to render it beneficial in the highest degree to medical science, a flora of its medicinal plants, many of which are limited to particular soils and elevations.

In the west the state of Tennessee, as we announced in a preceding number, has taken the lead in this particular application of science; but we are happy in being able say, that Kentucky, Indiana and Ohio are likely to follow her example; and would exhort our readers to use their influence with those to whom this subject is confided, till it shall be fully accomplished.

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